Philip Trueman

----- FUSION FORUM appended at 20:03:03 on 89/04/07 GMT (by NORD at MANCSP1) --
Subject: TV interview with Pons 4/6
Ref: Append at 22:15:27 on 89/04/06 GMT (by MMFARROW at ALMVMC)

Well, Nobody in the know has appended a summary of last night's WETA interview with Pons, so I'll give it a shot.

Overall review: Disappointing. Those of you "outside the beltway" missed very little.

Maybe I knew too much before the interview, but the technical aspects of the potential discovery were not sufficiently discussed.
Only the first 3 minutes of the interview discussed the experiment.
The remainder was spent asking Mr. Pons how he could prove his findings and asking him to refute the statements of some physicists who are skeptical of his experiment.

For me, knowing that the experiment is not confirmed is OK.
Enough said, lets move onto the technical aspects.
If the experiment is not reproducable, we will know soon enough.
A technical discussion of the experiment and the potential society benefits would have been quite nice.

Oh well, hopefully other interviews will follow.

--- Joe Nord SID/FS Manassas, VA

----- FUSION FORUM appended at 21:38:22 on 89/04/07 GMT (by PDC at SJEVM5) ----
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)

You mentioned using Pd foil. Pons has stated that foil won't work, you MUST use a rod?

Paul D. Chamberlain

----- FUSION FORUM appended at 23:23:29 on 89/04/07 GMT (by VENDOR26 at PALVMPSQ)
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 21:38:22 on 89/04/07 GMT (by PDC at SJEVM5)

You also mention a window in the cell. As Bob Miller mentioned earlier, we are dealing with very high D/Pd ratios. Couldn't the exposed side of the cathode allow deuterium to diffuse out into the air, dropping the D/Pd ration to a point where fusion won't occur ?

Dick Murtagh

OPTIONS: NOACK LOG LONG NOTEBOOK *
Local options: Search RealNode

Date: 9 April 1989, 12:33:30 EDT
From: RLG2 at YKTVMT
To: STEINH at PENNDRLS
cc: WPRESS at CFA
KOONIN at SBITP
Subject: Tritium in TiD, etc.

Paul, thanks for the suggestion about loading Pd or Ti lattices. Maurice Goldhaber has identified the sites on which the deuterons must sit as superstitial sites.

Seriously, the problem with looking for "independent" sites from which fusion must take place is that the oscillations in the wells must be anticorrelated when they would otherwise bring the deuterons to within 0.2 A of one another-- a repulsive Coulomb potential of 65 eV.

As for T in VSOP TiD, one just has to look at the numbers. At IBM we have run for days, certainly having saturated a 2 mg/sq cm film of Pd and looking for the protons from D + D = T + P. We should have seen 4000 and have not seen even one.

I am persuaded the explosive releases of energy come from electrochemically produced high explosive, with D occupying high energy sites in the lattice. This is NOT 10**27 atmospheres as per F-P, but about 40,000 atm at most-- essentially the energy per unit volume multiplied by a non-dimensional shape factor for the density of states vs. applied potential. But it is enough to destroy the cell and allow PdD reaction with air.

As for the steady release of heat, look at the tables to see that it is never bigger than the V x I applied to the cell (that is the excess is never more than 53%, as I recall). Catalytic recombination of the electrolytic O2 with the added D gas ("sparging") can account for some of that.

In any case, I will spend all day Wednesday in Erice with Fleischmann, Jones, Ponamarev, Gerschtein, and a number of experimenters from the U.S. Anybody want to come?

Dick Garwin

Date: 9 April 1989, 23:10:39 EDT
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: ZIEGLER at YKTVMV
Subject: Feeling peaked?

Re your peak of 10 counts "in 3 channels" at 6.2 MeV, what is the delta-E per channel? Is that narrow width compatible with isotropic emission from the foil? What would have to be the linear energy loss by the particle in traversing the PdH in order that the greater average loss at skew angles (or at greater initial depth) not spread the peak?

Thanks. Dick

WHAT'S NEW, Friday, 7 April 1989 Washington, DC

1. THIS MAY BE THE YEAR WE LEARN TO SPELL SEQUESTRATION. Some members of Congress are thinking about abandoning the effort to develop a FY 90 budget and simply allow sequestration to occur. With little more than a rough outline of a budget from the White House, Congress feels it has been left to them to do all the dirty work required to comply with the Gramm-Rudman-Hollings Deficit Reduction Act. GRH provides for a completely mechanical reduction in the budget to meet the established guidelines in case Congress fails to reach an agreement. The expectation was that Congress would find this prospect too awful to contemplate and thus seek agreement. But the mindlessness of sequestration can be appealing--no one's fingerprints show up on the axe. An informal Office of Management and Budget estimate predicts a 9.7% across-the-board cut of non-exempt, non-defense discretionary spending. That, for example, would cost the NSF nearly $200,000.

2. COLD-FUSION PAPERS WERE SCHEDULED FOR THE SPRING APS MEETING in Baltimore, May 1-4, weeks before the University of Utah began contacting financial publications. These include an invited paper on Wednesday morning by Steve Jones of Brigham Young University, titled "Cold Nuclear Fusion: Recent Results and Open Questions." The paper is part of a symposium organized by the Topical Group on Few-Body Systems and Multi-Particle Dynamics. This may lead to speculation of pre-cognition by the organizers. A special Monday evening session for presentation of new results on cold fusion is also being arranged. It could be the denouement. A quick survey of laboratories late today turned up no support for either the Utah or Brigham Young claims. A report in the press that Brookhaven scientists had evidence of fusion was incorrect.

3. THE FIRST COLD FUSION SHOOT OUT WILL TAKE PLACE AT HIGH NOON in Dallas next week at the American Chemical Society Meeting. Stanley Pons will be there in person at a special session on Wednesday, but a news conference planned for Wednesday has been cancelled. Two well-known electrochemists will speak in the session along with Harold Furth, who heads the hot fusion effort at Princeton. Meanwhile, copies of the paper submitted to the Journal of Electronanalytical Chemistry by Pons and Fleischmann have been spewing out of FAX machines all over the country.

4. A BARBED WIRE CURTAIN HAS DECENDED OVER TEXAS. Super collider supporters in the Lone Star State are kicking up a ruckus over foreign participation. Last week it was Rep. Bryant of Dallas. "If this is worth $5 billion to us, then the innovative technology that would flow from it should be used to benefit Americans and not be given away to other countries," he said. Today, Texas Governor Clements is meeting in Washington with National Security Advisor Brent Scowcroft and Defense Secretary Cheney to request an investigation of the national security implications of foreign participation in the super collider!

Robert L. Park (202) 232-0189 The American Physical Society

WHAT'S NEW, Friday, 7 April 1989 Washington, DC

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Robert L. Park (202) 232-0189 The American Physical Society

099AZ FAX Q1 dated 89/04/09 21:56:01 ..... Page 1

(2.1)

[illegible]

I have sent this just now to Ziegler,
JoAnn, please send it first ~~099A~~
thing by FAX to (1), (2), (3).

Thank you, RLG

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555
FAX: (914) 945-2141, Telex: 137456 IBMRESRCH YKHG UD,
BITNET: RLG2 at IBM.COM

April 9, 1989
(Via FAX to 9-011 (39-923) 86-9226) — (1)

Professor Antonino Zichichi
EMCSC Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY

04/10/89
RLG:
Copies to (1), (2) and (3) have been sent via FAX.
jtm

Dear Nino,

Here is Steve Koonin's name and address to add to the list of those expected. He is arriving Rome Tuesday morning, 04/11/89 at 0845 on PA 1110. I advised him to take BM 120 at 1035 for Palermo with me and Jim Ziegler. Koonin has written a paper with Mike Nauenberg on a refined calculation of reaction rate for D-D and P-D in a reasonable potential.

Because of the lower reduced mass, the P-D rate is much faster than the D-D.

Dr. Steven E. Koonin
Kellogg Radiation Lab. 106-38
California Institute of Technology
Pasadena, CA 91125

W: (818) 356-4586
FAX: (818) 795-1547

Please note that Koonin is on leave in Santa Barbara:

Dr. Steven E. Koonin
Institute of Theoretical Physics
University of California
Santa Barbara, CA 93106

H: (805) 563-0832
BITNET: KOONIN at SBITP

On another subject, could Jerry Pilarsky please arrange for Profs. Jones and Fleischmann each to have someone available at their respective laboratories on Wednesday, 04/12/89, from noon Erice time until 6 pm (in Utah that will be from about 0400 until 1000) with telephone and FAX right next to one another (on separate lines), so that we could communicate with them from Erice and get their responses? The World Laboratory could pay for rental of a FAX machine from a local dealer at about $100 for a month, if the labs don't have a local FAX.

I'll be at a meeting in New York City Monday morning, so it will really be easier to do this from Erice than for me to try to accomplish it.

See you Tuesday, if all goes well.

Sincerely yours,

Richard L. Garwin

cc:
  A. Zichichi, (via FAX to CERN 9-011-(41-22)-82-7774) —(2)
  J.F. Ziegler, IBM (via VNET to ZIEGLER).
  T.D. Lee, via FAX to Columbia Physics 9 (212) 932-3169 —(3)

RLG:rlg:099%AZ:040989..AZ

Richard L. Garwin
IBM Research Division
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P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555
FAX: (914) 945-2141, Telex: 137456 IBMRESRCH YKHG UD, BITNET: RLG2 at IBM.COM

April 7, 1989
(Via FAX to 9-011-(44-1)-727-8625)
Did not go through by FAX so instead sent to Moscow:
(Via Telex to 871411964)

Academician V.I. Goldanskii
Chairman, Soviet Pugwash Committee
Academy of Sciences of the USSR
Moscow
USSR

Dear Vitaly,

I write on the chance that this will reach you in London, where I know you had dinner Wednesday night with Laura.

There is a meeting in Erice, Sicily, Wednesday 04/12/89 on the subject of cold fusion. Martin Fleischmann will be there, and Steven Jones. In addition, there will be other interested people, such as myself, Jim Ziegler from IBM, Matthijs Broer from Bell Labs, probably Rulon Linford from Los Alamos, and maybe one or two others from the United States.

From the Soviet Union, there will be Ponamarov and Gerschtein.

Knowing your interest in low-temperature chemistry, I am sure that we would benefit greatly by your participation. The idea is to arrive in Rome on Tuesday and fly to Palermo, from which transport will be provided to Erice. Accommodation will be provided in Erice and travel back to Palermo on Thursday. You could also stay several days longer in Erice, if you wished.

Nino Zichichi will reimburse travel expenses including business class airfare.

I look forward to seeing you in Erice on Wednesday, if you can arrange your schedule to be there.

Very best regards.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

RLG:jtml:097%VIG:040789.VIG

04/07/89

CFE [OTH + SBR ??

[illegible] - metal rods'; tags & seals, 1900

172-1022

9828

Allento

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-------------------------------------------------------------------------------
Date:    Fri, 7 Apr 89 11:23:24 PDT
From:     koonin@sbitp.bitnet
Message-Id: <890407112324.3f37@sbitp.ucsb.edu>
Subject: cold fusion preprint
To:       rlg2@yktvmv.bitnet, rlg2%ibm.com@sbitp.bitnet
X-ST-Vmsmail-To: ST%"rlg2@yktvmv.bitnet", ST%"rlg2@ibm.com"
```

April 7, 1989

There follows the TeX script of a preprint on Cold Fusion that might be of interest to you. Please feel free to distribute it (either electronically or in hard copy) to others who might be interested.

Steve Koonin █KOONIN@SBITP.BITNET█

```
█magnification=1200
█voffset=1 true in
█vsize=8.9 true in
█hoffset=2.5truecm
█hsize=6.5 true in

█baselineskip=24pt plus .5pt minus .5pt


█overfullrule=0pt

█centerline██bf Cold fusion in isotopic hydrogen molecules█
█medskip
█centerline█S. E. Koonin$██¬*$ and M. Nauenberg$██¬█**█$█
█medskip
█centerline██it Institute for Theoretical Physics█
█centerline██it University of California█
█centerline██it Santa Barbara, CA  93106█
█centerline█Submitted to ██it Nature█, April 7, 1989█

█bigskip

██narrower█narrower█smallskip
We have calculated cold fusion rates in diatomic hydrogen molecules
involving various isotopes.  An accurate Born-Oppenheimer potential was
used to calculate the ground state  wave functions.  We find that the
rate for $█rm d+d$ fusion is $3 █times10¬█-64██,█rm s¬█-1█$, some 10
orders of magnitude faster than a previous estimate.  We also find that
the rate for $█rm p+d$ fusion is $10¬█-55██,██rm  s█¬█-1█$, which is
larger than $█rm d+d$ due to the enhanced tunneling in the lighter
system.  Enhancements of the electron mass by factors of 5--10 would be
required to bring cold fusion rates into the range of recently claimed
observations.
█smallskip█

█bigskip
█goodbreak

██Cold fusion'' (CF) occurs when two nuclei with very small relative
energy tunnel through their mutual coulomb barrier to initiate a nuclear
reaction. The phenomenon is well-studied in muon catalyzed
fusion█ 1,2,3█, where the large mass of the muon relative to an electron
results in tightly-bound diatomic muo-molecules of Hydrogen (e.g.,
d-$█mu$-t) with cold fusion rates of order $10¬█12██;██rm s█¬█-1█$. It
is also believed to occur as pycno-nuclear reactions in certain
astrophysical environments█ 4█.  Recent reports of CF between hydrogen
```

$1\,eV \times 5\times10^{22}\,cm^{-3}$

1.6×10^{-12}

$= 8\times10^{10}\,cm^{-3}$

$\overline{\quad 10^{7}}$

$= 8\,kJ/cm^3$

say 5h watt/cm

on $\pi/4 \times 0.4$ cm

$\frac{\pi}{4}\times(0.4)^2$

2.048×10

$= 20\,A/cm^3$

at 1V (to set scale)

$4\,MJ/cm^3$

is $\frac{W}{I} = \frac{4\times10^6}{20}$

$= 2\times10^5$ sec

2 days

```
isotopes embedded in Palladium█[5█ and Titanium metal█[6█ have prompted
us to reconsider previous estimates of the CF rates for free diatomic
isotopic hydrogen molecules.

Consider a free diatomic molecule composed of two hydrogen nuclei (which
might be different isotopes).  In the Born-Oppenheimer approximation the
fusion rate $█Lambda$
is proportional to the probability that the nuclei are very
close together:
$$
█Lambda = A |█Psi ( █rho )|¬2█;,
█eqno(1)
$$
where $█Psi$ is the normalized wave function describing their relative
motion The inter-nuclear separation $█rho$ is a typical distance where
nuclear interactions occur, approximately 10█fm.

The nuclear rate constant $A$ for a given pair of nuclei is related to
the low-energy behavior of the corresponding fusion cross section.  If
the variation of the cross section $█sigma(E)$  with relative energy $E$
is parameterized in terms of the usual S-factor,
$$
█sigma (E) = █S(E) █over E█ e¬ █-2 █pi █eta █; █ █ █
█eta=█e¬2 █over █(2E█hbar¬2 /█mu)¬█1/2██ ██;,
█eqno(2)
$$
with $█mu$ the reduced mass of the two nuclei,
then
$$
A = █S(E=0) █over ██mu c¬2██ █  █c █over ██pi █alpha ██
█eqno(3)
$$
with $█alpha=e¬2/█hbar c █approx 1/137$.  Table█1 shows the nuclear rate
constants for four possible interactions between two hydrogen
nuclei█[7█.

We restrict ourselves only to s-wave nuclear motion (for which the
fusion rate will be largest), so that the wave function can be written
as
$$
█Psi (r) =   ██psi(r) █over █4█pi r██
█eqno(4)
$$
with the normalization $█int_0¬█infty █psi¬2 █,dr = 1$. The radial wave
function $█psi$ then satisfies the Schroedinger equation
$$
█- ██hbar¬2 █over █2 █mu██ █d¬2 █over dr¬2█  + V(r)█ █psi(r) = █epsilon
█psi(r)
█eqno(5)
$$
where $█epsilon$ is the eigenvalue for relative motion.  Unless
otherwise specified, we will hereafter work in atomic units $(e¬2 =
█hbar = m_e = 1)$, so that all energies are measured in Hartrees
($█approx 27.2 █;██rm eV█$) and all distances are measured in Bohr radii
($a █approx 0.53 █times 10¬█-8██;██rm cm█$).

Simple considerations determine the general features of $V(r)$.  If
energies are measured relative to the energy of two isolated Hydrogen
atoms $(-1)$, $V$ vanishes at large $r$.  Further, it must have a
minimum of the proper depth and separation to support the observed
molecular bound states.  At small separations, the electronic structure
```

```
is that of the He atom  with an energy of $V_0 = - 1.9037$ relative to
two isolated hydrogen atoms, so that
$$
V(r ■to 0) ■to ■1 ■over r■ + V_0■; .
■eqno(6)
$$

An estimate of the supression of the fusion rate by tunneling is given
by the barrier penetration factor obtained from the WKB approximation to
Eq. (5):
$$
                B=e¬■-2 ■int_0¬■r_0■ k(r) dr■
■eqno(7)
$$
where the local wavenumber is $k(r) = [2■mu (V(r) - ■epsilon )]¬■1/2■$
and the integral extends to the classical turning point, $r_0$.

To estimate the barrier penetration integral (7), we have taken for the
diatomic molecular potential $V(r)$ the current best available numerical
calculation in the Born-Oppenheimer approximation done by Kolos and
Wolniewicz (K--W)■[8,9]. For $1.1 < r <3$ this potential is well
approximated by the Morse potential
$$
         V(r)=0.1745 [e¬■-2.08(r-1.4)■ -2 e¬■-1.04(r-1.4)■]
■eqno(8)
$$
For smaller values of $r$ we fitted the calculated values of $V - 1/r$ to a
seven term Lagrange interpolation formula. Upon numerical evaluation of
the integral using the exact $d + d$ eigenvalue and turning point,
we find
$$
           B = e¬■-4.13 ■sqrt■mu■
■eqno(9)
$$
The numerical coefficient of 4.13 is to be compared with the estimate of
3.0--3.3 made by Zeldovich and Gershtein [2]; the difference leads
to a penetration factor which is 15-21 orders of magnitude
smaller.

An accurate evaluation of the CF rates can be obtained by a direct
numerical integration of the Schroedinger equation■(5) with the K--W
potential, as shown in the second column of Table■2. The nuclear radius
was taken as $■rho = 2 ■times 10¬■-4■ (■approx 10■,■■rm fm■$). The
numerical methods of■[10] were used, with the wave function being treated
explicitly only for $r>0.005$; the regular s-wave Coulomb function was
used to extrapolate this solution to $r=■rho$.

The exact dependence of the barrier factor on reduced mass that we
extract from these results is good agreement with the WKB approximation
(7), but disagrees with references [2] and [11]. The estimate of the
$■rm d+d$ fusion rate made in [11] is too low by about 10 orders of
magnitude, because in the calculation of the WKB penetration integrals
an unshifted coulomb potential was used at small separations (i.e., our
Eq.■(6) with $V_0 = 0$). The effective energy with which the nuclei
assault the coulomb barrier is therefore lower than it should be, and
hence the calculated fusion rate is smaller. We note that the $■■rm
p+d■$ fusion rate is 7.5 orders of magnitude larger than the $■rm d+d$ rate.
Although the nuclear rate constant for $■rm p+d$ is some 5.5 orders of
magnitude smaller than for $■rm d + d$, the smaller reduced mass
enhances the tunneling probability more than enough to compensate for
this.
```

But one wants Psi, not psi, so Psi ~ psi/r and the Psi² is larger (multiplied) than psi² by a factor $\sim \left(\frac{10^{-8}}{3\times10^{-13}}\right)^2 \sim 10^9$ no?

or if $x = 2\times10^{-4}$ $(5\times10^3)^2 = 2.5\times10^7$

How so??

```
It is interesting to ask by how much the internuclear separation must be
decreased in order to reach the fusion rates claimed in [5,6]. Although
the precise answer depends upon the details of the internuclear
potential, a simple way of quantifying the problem is to endow the
electron with a larger mass $m¬*$ than it actually has.  The equilibrium
internuclear separation then scales as $m_e/m¬*$, while Eq.■(9) above
allows the enhancement to be estimated as
$$
■log_■10■ ■,[■Lambda(m¬*)/■Lambda(m_e)] = 3 ■log_■10■ ■,(m¬*/m_e) -79
(■mu/M_n)¬■1/2■ [(m_e/m¬*)¬■1/2■-1]■;,
■eqno(10)
$$
where $M_n$ is the nucleon
mass and the logarithmic variation with $m¬*$ is due to the scaling of
$■Psi (■rho)$. More accurate estimates can be had by numerical
integration of the Schroedinger equation (5) with the K--W potential
[8,9], as shown in Table■2.

Note that a mass enhancement of $m¬* ■approx 5m_e$ would be required to bring
the CF rates into the range claimed by [6] while $m¬* ■approx 10m_e$ is
required by the results of [5].  These should be taken as only a rough
guide, however, as Hydrogen in Palladium is dissociated into atoms and
ionized to bare nuclei [12].

It is worth remarking on the validity of the Born-Oppenheimer
approximation we have used in our calculations.  Since there is
a large difference between the potential and total energies in the
classically forbidden region, one might naively expect a failure of the
adiabatic assumption. However, more careful reflection suggests
otherwise. Sytematic corrections to the adiabatic approximation are
possible by considering the full coupled-channels generalization of
Eq.■(5) [13].  The largest coupling terms are of order
$$
■1 ■over ■mu■ ■langle n| ■■partial ■over ■partial r■ |m■rangle
■d■Psi_m (r) ■over dr ■ ■approx ■k(r) ■over ■mu■ ■Psi_m (r) ■;,
■eqno(11)
$$
where $n,m$ label the electronic states, which we assume to vary on the
scale of the Bohr radius. The local wavenumber at small distances is
$k(r) ■approx (2 ■mu /r)¬■1/2■ $. This term is to be compared with the
effect of the
diagonal potential at short distances, $■approx ■Psi_m /r$, giving a
correction to adiabaticity of order $(k/■mu)/(1/r) ■approx (r
/■mu)¬■1/2■ ■ll 1$.

In summary, we have calculated cold fusion rates in isotopic Hydrogen molecules.

We find that the rate for $■rm d+d$ fusion in the free molecule
($■approx 3 ■times 10¬■-64■■,■■rm s■¬■-1■$) is some 10■orders of
magnitude larger than the most recent previous estimate [11], but that
the rate for $■rm p+d$ is faster yet by some 8 orders of magnitude.
This latter remains true for rates slower than $■approx 3 ■times
10¬■-17■■, ■■rm s■¬■-1■$  ($m¬* ■approx 6 m_e$). Hence, if refs.  5,6] are
seeing any nuclear process at all, it is more likely the neutron-free
$■rm p+d$ reaction rather
than $■rm d+d$. We also find that hypothetical enhancements of the
electron mass by factors of 5--10 are required to bring CF rates into
the range of values claimed experimentally. However, we know of no
plausible mechanism for achieving such enhancements.
■bigskip
```

```
We would like to thank D.█Eardley and many of our other colleagues at
the ITP for fruitful discussions.  We are also grateful to B. Kirtman
for a numerical calculation of the diatomic potential at $r=0.1$.  This
work was support in part by National Science Foundation grant
PHY82-17853 at Santa Barbara, supplemented by
NASA funds,  and by National Science Foundation grants
PHY86-04197 and PHY88-17296 at Caltech.
█vfill█eject
█centerline██bf References█
█frenchspacing
█medskip
█item█*█ Permanent address: W. K. Kellogg Radiation Laboratory, Caltech
106-38, Pasadena, CA 91125
█item█**█ Permanent address: Physics Dept. and Institute of Nonlinear
Sciences, University of California, Santa Cruz, CA 95064

█item█1█ J. D. Jackson,  Phys. Review ██bf106█ (1957) 330.

█item█2█ Ya. B. Zel'dovich and S. S.Gershtein, Soviet Physics Uspekhi
██bf3█ (1961) 593.

█item█3█ J. Rafelski and S. E. Jones, Scientific American ██bf 257█
(July, 1987) 84.

█item█4█ S. L. Shapiro and S. L. Teukolsky, Black Holes, White Dwarfs and
Neutron Stars (Wiley, New York, 1983) p.█72.

█item█5█ M. Fleischmann and S. Pons, University of Utah preprint, March, 1989.



█item█6█ S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen,
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Cold fusion in isotopic hydrogen molecules

S. E. Koonin* and M. Nauenberg**

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Submitted to *Nature*, April 7, 1989

We have calculated cold fusion rates in diatomic hydrogen molecules involving various isotopes. An accurate Born-Oppenheimer potential was used to calculate the ground state wave functions. We find that the rate for d + d fusion is $3 \times 10^{-64}\,s^{-1}$, some 10 orders of magnitude faster than a previous estimate. We also find that the rate for p + d fusion is $10^{-55}\,s^{-1}$, which is larger than d + d due to the enhanced tunneling in the lighter system. Enhancements of the electron mass by factors of 5–10 would be required to bring cold fusion rates into the range of recently claimed observations.

"Cold fusion" (CF) occurs when two nuclei with very small relative energy tunnel through their mutual coulomb barrier to initiate a nuclear reaction. The phenomenon is well-studied in muon catalyzed fusion [1,2,3], where the large mass of the muon relative to an electron results in tightly-bound diatomic muo-molecules of Hydrogen (e.g., d-μ-t) with cold fusion rates of order 10^{12} s^{-1}. It is also believed to occur as pycno-nuclear reactions in certain astrophysical environments [4]. Recent reports of CF between hydrogen isotopes

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embedded in Palladium [5] and Titanium metal [6] have prompted us to reconsider previous estimates of the CF rates for free diatomic isotopic hydrogen molecules.

Consider a free diatomic molecule composed of two hydrogen nuclei (which might be different isotopes). In the Born-Oppenheimer approximation the fusion rate Λ is proportional to the probability that the nuclei are very close together:

$$\Lambda = A|\Psi(\rho)|^2 , \tag{1}$$

where Ψ is the normalized wave function describing their relative motion The inter-nuclear separation ρ is a typical distance where nuclear interactions occur, approximately 10 fm.

The nuclear rate constant A for a given pair of nuclei is related to the low-energy behavior of the corresponding fusion cross section. If the variation of the cross section $\sigma(E)$ with relative energy E is parameterized in terms of the usual S-factor,

$$\sigma(E) = \frac{S(E)}{E} e^{-2\pi\eta}; \quad \eta = \frac{e^2}{(2E\hbar^2/\mu)^{1/2}} , \tag{2}$$

with μ the reduced mass of the two nuclei, then

$$A = \frac{S(E=0)}{\mu c^2} \, \frac{c}{\pi\alpha} \tag{3}$$

with $\alpha = e^2/\hbar c \approx 1/137$. Table 1 shows the nuclear rate constants for four possible interactions between two hydrogen nuclei [7].

We restrict ourselves only to s-wave nuclear motion (for which the fusion rate will be largest), so that the wave function can be written as

$$\Psi(r) = \frac{\psi(r)}{4\pi r} \tag{4}$$

with the normalization $\int_0^\infty \psi^2 \, dr = 1$. The radial wave function ψ then satisfies the Schroedinger equation

$$[-\frac{\hbar^2}{2\mu}\frac{d^2}{dr^2} + V(r)]\psi(r) = \epsilon\psi(r) \tag{5}$$

where ϵ is the eigenvalue for relative motion. Unless otherwise specified, we will hereafter work in atomic units ($e^2 = \hbar = m_e = 1$), so that all energies are measured in Hartrees (≈ 27.2 eV) and all distances are measured in Bohr radii ($a \approx 0.53 \times 10^{-8}$ cm).

Simple considerations determine the general features of $V(r)$. If energies are measured relative to the energy of two isolated Hydrogen atoms (-1), V vanishes at large r. Further, it must have a minimum of the proper depth and separation to support the observed molecular bound states. At small separations, the electronic structure is that of the He atom with an energy of $V_0 = -1.9037$ relative to two isolated hydrogen atoms, so that

$$V(r \to 0) \to \frac{1}{r} + V_0 \; . \tag{6}$$

An estimate of the supression of the fusion rate by tunneling is given by the barrier penetration factor obtained from the WKB approximation to Eq. (5):

$$B = e^{-2\int_0^{r_0} k(r)dr} \tag{7}$$

where the local wavenumber is $k(r) = [2\mu(V(r) - \epsilon)]^{1/2}$ and the integral extends to the classical turning point, r_0.

To estimate the barrier penetration integral (7), we have taken for the diatomic molecular potential $V(r)$ the current best available numerical calculation in the Born-Oppenheimer approximation done by Kolos and Wolniewicz (K–W) [8,9]. For $1.1 < r < 3$ this potential is well approximated by the Morse potential

$$V(r) = 0.1745[e^{-2.08(r-1.4)} - 2e^{-1.04(r-1.4)}] \tag{8}$$

For smaller values of r we fitted the calculated values of $V - 1/r$ to a seven term Lagrange interpolation formula. Upon numerical evaluation of the integral using the exact $d + d$ eigenvalue and turning point, we find

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An accurate evaluation of the CF rates can be obtained by a direct numerical integration of the Schroedinger equation (5) with the K–W potential, as shown in the second column of Table 2. The nuclear radius was taken as $\rho = 2 \times 10^{-4} (\approx 10\,\mathrm{fm})$. The numerical methods of [10] were used, with the wave function being treated explicitly only for $r > 0.005$; the regular s-wave Coulomb function was used to extrapolate this solution to $r = \rho$.

The exact dependence of the barrier factor on reduced mass that we extract from these results is good agreement with the WKB approximation (7), but disagrees with references [2] and [11]. The estimate of the d + d fusion rate made in [11] is too low by about 10 orders of magnitude, because in the calculation of the WKB penetration integrals an unshifted coulomb potential was used at small separations (i.e., our Eq. (6) with $V_0 = 0$). The effective energy with which the nuclei assault the coulomb barrier is therefore lower than it should be, and hence the calculated fusion rate is smaller. We note that the p + d fusion rate is 7.5 orders of magnitude larger than the d + d rate. Although the nuclear rate constant for p + d is some 5.5 orders of magnitude smaller than for d + d, the smaller reduced mass enhances the tunneling probability more than enough to compensate for this.

It is interesting to ask by how much the internuclear separation must be decreased in order to reach the fusion rates claimed in [5,6]. Although the precise answer depends upon the details of the internuclear potential, a simple way of quantifying the problem is to endow the electron with a larger mass m^* than it actually has. The equilibrium internuclear separation then scales as m_e/m^*, while Eq. (9) above allows the enhancement to be estimated as

$$\log_{10}[\Lambda(m^*)/\Lambda(m_e)] = 3\log_{10}(m^*/m_e) - 79(\mu/M_n)^{1/2}[(m_e/m^*)^{1/2} - 1] , \qquad (10)$$

where M_n is the nucleon mass and the logarithmic variation with m^* is due to the scaling of $\Psi(\rho)$. More accurate estimates can be had by numerical integration of the Schroedinger equation (5) with the K–W potential [8,9], as shown in Table 2.

Note that a mass enhancement of $m^* \approx 5m_e$ would be required to bring the CF rates into the range claimed by [6] while $m^* \approx 10m_e$ is required by the results of [5]. These should be taken as only a rough guide, however, as Hydrogen in Palladium is dissociated into atoms and ionized to bare nuclei [12].

It is worth remarking on the validity of the Born-Oppenheimer approximation we have used in our calculations. Since there is a large difference between the potential and total energies in the classically forbidden region, one might naively expect a failure of the adiabatic assumption. However, more careful reflection suggests otherwise. Sytematic corrections to the adiabatic approximation are possible by considering the full coupled-channels generalization of Eq. (5) [13]. The largest coupling terms are of order

$$\frac{1}{\mu}\langle n|\frac{\partial}{\partial r}|m\rangle \frac{d\Psi_m(r)}{dr} \approx \frac{k(r)}{\mu}\Psi_m(r) , \qquad (11)$$

where n, m label the electronic states, which we assume to vary on the scale of the Bohr radius. The local wavenumber at small distances is $k(r) \approx (2\mu/r)^{1/2}$. This term is to be compared with the effect of the diagonal potential at short distances, $\approx \Psi_m/r$, giving a correction to adiabaticity of order $(k/\mu)/(1/r) \approx (r/\mu)^{1/2} \ll 1$.

In summary, we have calculated cold fusion rates in isotopic Hydrogen molecules. We find that the rate for d + d fusion in the free molecule ($\approx 3 \times 10^{-64}\,\mathrm{s}^{-1}$) is some 10 orders of magnitude larger than the most recent previous estimate [11], but that the rate for p+d is faster yet by some 8 orders of magnitude. This latter remains true for rates slower than $\approx 3 \times 10^{-17}\,\mathrm{s}^{-1}$ ($m^* \approx 6m_e$). Hence, if refs. [5,6] are seeing any nuclear process at all, it is more likely the neutron-free p+d reaction rather than d+d. We also find that hypothetical enhancements of the electron mass by factors of 5–10 are required to bring CF rates into the range of values claimed experimentally. However, we know of no plausible mechanism for achieving such enhancements.

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Table 1: Rate constants for fusion of hydrogen isotopes

Reaction	$S(E=0)(\mathrm{Mev}-\mathrm{b})$	$A(\mathrm{cm}^3\,\mathrm{s}^{-1})$
$\mathrm{p}+\mathrm{d}\rightarrow {}^3\mathrm{He}+\gamma$	2.5×10^{-7}	5.2×10^{-22}
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$\mathrm{d}+\mathrm{t}\rightarrow {}^4\mathrm{He}+\mathrm{n}$	1.1×10^{1}	1.3×10^{-14}

Table 2: CF rates in isotopic hydrogen molecules

(Entries are $\log_{10}$ of the rate is s^{-1})

	$m^*/m_e=1$	2	5	10
$\mathrm{p}+\mathrm{d}$	−55.0	−36.0	−19.0	−10.4
$\mathrm{p}+\mathrm{t}$	−57.8	−37.7	−19.7	−10.5
$\mathrm{d}+\mathrm{d}$	−63.5	−40.4	−19.8	−9.1
$\mathrm{d}+\mathrm{t}$	−68.9	−43.5	−20.9	−9.4

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Date: 7 April 1989, 00:42:56 EDT
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: ZIEGLER at YKTVMV
Cc: RSA1 at YKTVMV
Subject: Thanks, etc.

Thanks for all the information. I am hereby asking JoAnn McLoughlin to get Tyson Broer's FAX number at AT&T Bell Labs by calling him at (201) 582-7761 to fill him in on the Erice meeting (by sending him the FAXs I have sent Zichichi.

I agree with the judgment that it makes no sense to go to Dallas. I guess we never got the FAX Nino said he had sent me Thursday at 0800 Lausanne time, but Nino insists Fleischmann will be in Erice.

I'll call Rulon Linford Friday morning to see whether I can get him to come.

What news on the Harwell "announcement"?

I'll talk with you over the weekend about our transport to and from JFK Monday and Wednesday. Now I'll FAX Zichichi to ensure that THEY FAX the participants to tell them of the meeting, the provision of airfare (business class, etc.)

On the muon-catalyzed D2O, I had thought about that.

a) Even in liquid D2, the yield averages less than one fusion per muon.
b) The Fermi-Teller "Z law" says that only 1/4 of the muons will initially lodge on a D, but it will then diffuse "as a neutron" through the solid and transfer to an O before it finds another D. This loss from the nucleus is peculiar to Z = 1.
c) Even if one got a fusion, the liberated muon would have the a,b problem all over again.

So the yield will be VERY small.

I know that you will help Mrs. McLoughlin during my absence Friday if she calls on you.

Dick

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via FAX to 9-516-282-3000)

Dr. Richard Hahn
Brookhaven National Laboratory
Building 555A
Upton, NY 11973

Dear Dr. Hahn:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

Professors Martin Fleischmann and Steven Jones have committed to participate on Wednesday.

In our discussions, we hope to understand the evidence for cold fusion, any alternative explanations or inconsistencies, and to determine where one might go from here.

If you can purchase your air ticket (business class), the Center will provide reimbursement for the air fare as well as for other travel costs. We can also provide a prepaid ticket, but time is very short. If you need a prepaid ticket, please FAX the request to (39-923) 86-2226 or call Dr. Jerry Pilarsky (39-923) 86-9133.

Please reply to Richard L. Garwin at (914) 945-2555 or via FAX (914) 945-2141.

Sincerely yours,

Antonino Zichichi

RLG:jtml:097%RH:040789..RH

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

April 7, 1989
(Via FAX to (301) 353-2791)

Dr. Rulon K. Linford
c/o Anne Davies (DOE Washington)
Los Alamos National Laboratory
P.O. Box 1663
Los Alamos, NM 87545

Dear Dr. Linford:

I have been at a meeting at the U.S. Space Foundation in Colorado Springs, and yesterday my flight made an emergency landing in Dayton, so my ability to communicate with you by telephone has been minimal. I want to bring you up to date on the arrangements for the meeting in Erice.

The meeting will definitely take place Wednesday, 04/12/89. In addition to Fleischmann and Jones, there will be Ziegler from IBM, Tyson Broer from Bell Labs, myself, and from the Soviet Union, Ponamarov and Gerschtein. I will also try to twist the arm of Charles Barnes, but I don't know that he will be able to make it.

I will try also to bring Vitaly Goldanskii if I can find him in time.

We would like very much for you to be there. Would you please call my office-- Mrs. McLoughlin-- at (914) 945-2555 to let us know definitely.

Your travel (business class) will be reimbursed, and transportation will be provided from the airport in Palermo to Erice and return, as well as accommodations in Erice.

It should be a very interesting meeting, and should be fun, too.

Sincerely yours,

Richard L. Garwin

Richard L. Garwin
Forwarded in his absence

RLG:jtml:097%RKL:040789.RKL

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via FAX to (505) 667-1754)

Dr. Rulon K. Linford
Los Alamos National Laboratory
P.O. Box 1663
Los Alamos, NM 87545

Dear Dr. Linford:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

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Please reply to Richard L. Garwin at (914) 945-2555 or via FAX (914) 945-2141.

Sincerely yours,

Antonino Zichichi

RLG:jtml:097%RKL1:040789RKL1

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via FAX to (818) 795-1547)

Professor Charles A. Barnes
California Institute of Technology
Kellogg Laboratory
1201 East California
Pasadena, CA 91125

Dear Professor Barnes:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

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Please reply to Richard L. Garwin at (914) 945-2555 or via FAX (914) 945-2141.

Sincerely yours,

Antonino Zichichi

RLG:jtml:097%CAB:040789.CAB

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via FAX to (201) 582-2913)

Dr. Matthias Broer
AT&T Bell Laboratories
600 Mountain Avenue
Room 6E222
Murray Hill, NJ 07974

Dear Dr. Broer:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

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Please reply to Richard L. Garwin at (914) 945-2555 or via FAX (914) 945-2141.

Sincerely yours,

Antonino Zichichi

RLG:jtml:097%MB:040789..MB

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via VNET to ZIEGLER)

Dr. James F. Ziegler
IBM T.J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598

Dear Dr. Ziegler:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

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Please reply to Richard L. Garwin at (914) 945-2555 or via FAX (914) 945-2141.

Sincerely yours,

Antonino Zichichi

RLG:jtml:097%JFZ:040789.JFZ

Date: 6 April 1989, 10:14:44 EDT
From: James F. Ziegler 8-862-2165 ZIEGLER at YKTVMX
IBM - Research (28-024) ATT: 914-945-2165
Yorktown, New York, 10598

To: Richard Garwin 862-2555 RLG2 at YKTVMX

Subject: Fusion.

Re: Note from you, sent on 04/05 at 07:54:44

I have talked to Bob Landise, manager of Material Chemistry at Bell Labs, about CNF. The Bell representative to Sicily could be Leonard Feldman, tele: 201-582-5470. He is at Syracuse University until Monday, and I have a call in to him to get his computer ID. At Syracuse he is visiting Dr. Vook, 315-443-2564.

Landise told me that the American Chemical Society meeting with Pons and Fleischmann on February 12 is absolutely certain. His people initiated the meeting and Bell is sending 4 people just for that day. I am getting an IBM chemist to reconfirm. But he was so insistent that I am making airplane reservations to Dallas instead of Sicily on April 11. Let me know if I should change directions.

Jim Ziegler

6 APR 89 12 07

R.L. GARWIN

Columbia University in the City of New York
DEPARTMENT OF PHYSICS

New York, N.Y. 10027
538 West 120th Street

COLUMBIA UNIVERSITY THEORETICAL PHYSICS GROUP

FACSIMILE COVER SHEET

2555

Dear Professor Garwin:

This is an information copy for your file.

TO: PROF. RICHARD GARWIN

FROM: PROF. T. D. LEE

DATE: 4/6/89

NUMBER OF PAGES TO FOLLOW: 1

OUR FAX NUMBER IS 212-932-3169

Columbia University in the City of New York | *New York, N.Y. 10027*
DEPARTMENT OF PHYSICS
598 West 120th Street

April 6, 1989

Professor A. Zichichi, Director
ICSC World Laboratory

Dear Professor Zichichi:

I have just received word from Brookhaven Laboratory that their representative to the Erice meeting will be

Dr. Richard Hahn
Office Phone: 516-282-4337
Home Phone: 516-874-2380
Home Address: 110 Crystal Beach Boulevard
Moriches, NY 11955.

Dr. Hahn is expecting to be contacted by your office regarding the details of the trip. He has suggested the following itinerary:

4/10 (evening) Departure for Erice (arrival 4/11)
4/13 [illegible]
4/14 or 4/15 Return to New York.

Yours sincerely,

Irene Tramm
(for Professor T.D. Lee)

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555
FAX: (914) 945-2141, Telex: 137456 IBMRESRCH YKHG UD,
BITNET: RLG2 at IBM.COM

April 6, 1989
(Via FAX to 9-011 (39-923) 86-9226)

Professor Antonino Zichichi
EMCSC Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY

Dear Nino,

I believe that you have not sent any formal invitations to the meeting in Erice. As for the U.S. participants I have contacted, I am willing for my office to communicate on your behalf your words of invitation and commitment to pay travel expenses.

If you will send me immediately a FAX from your organization reading something like

> "On behalf of the World Lab and of the ERICE Center, I ~~urge~~ am pleased to invite you ~~to participate~~ in the Fusion Forum at ~~the Center at Erice...~~ Wednesday, April 12. 11 April Please ~~plan~~ to travel from Rome to Palermo on Tuesday. the Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.
>
> Professors Martin Fleischmann and Steven Jones have committed to participate on Wednesday.
>
> In our discussions, we hope to understand the evidence for cold fusion, any alternative explanations or inconsistencies, and to determine where one might go from here.
>
> If you can purchase your air ticket (business class), the Center will provide reimbursement for the air fare as well as for other travel costs. We can also provide a prepaid ticket, but time is

OK

~~Italy~~ 39-923-86·2226

PAGE 2

very short. If you need a prepaid ticket, please FAX the request to ~~me~~ or call Dr Jerry BILARSKY - 39-923-869133 ~~him/her ??? at (39-???) ??-????."~~

I MUST have this communication if we are going to have people really attend. I am still in Colorado Springs, but my secretary, Mrs. McLoughlin will receive the FAX and do what is necessary, with the help and advice of Dr. Ziegler.

Thus far, we have myself and Ziegler, probably Tyson Broer from AT&T Bell Labs and Rulon Linford from Los Alamos, and probably not Charles Barnes from CalTech. I might be able to get a person from Brookhaven.

Ciao.

Sincerely yours,

Richard L. Garwin

cc:
A. Zichichi, (via FAX to CERN 9-011-(41-22)-82-7774)
J.F. Ziegler, IBM (via VNET to ZIEGLER).
T.D. Lee, via FAX to Columbia Physics 9 (212) 932-3169

RLG:rlg:096%AZ040689.AZ

Linford
Ziegler
Broer
Barnes
Richard Hahn
(Brookhaven)
FAX Ziegler

Dear Dick,
Please send the
above invitations
on my behalf. Nino

```
095A2     FAX       A1  V 80  Trunc=80 Size=133 Line=16 Col=1 Alt=0
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Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555
FAX: (914) 945-2141, Telex: 137456 IBMRESRCH YKHG UD,
BITNET: RLG2 at IBM.COM

April 5, 1989
(Via FAX to 9-011 (39-923) 86-9226) ✓

Professor Antonino Zichichi
EMCSC Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY

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====>
                                                            X E D I T  1 File
                                                                       IDLE

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Dear Nino,

Nino, re your proposed Fusion Forum in Erice 04/12/89 (arrive Rome 04/11, work all day in Erice 04/12), I report that it does not look good. But I have a constructive suggestion.

I have talked with Louis Rosen at Los Alamos (but not yet with Rulon Linford, who is coordinating the work there), with Maurice Goldhaber and Nick Samios at Brookhaven, with Will Happer at Princeton, just now with Charles Barnes at

```
  CalTech,  with Jim  Ziegler at  IBM, and  with many  others.
  Nobody has  seen a  neutron or gamma  ray, and  certainly no
  heating,  so they  are reluctant  or downright  unwilling to
  come to Erice now.

  Jim  Ziegler  and  I   would  participate,  but  that  would
  certainly not be ade   would  participate,  but  that  would
  certainly not be adequate  representation.  My suggestion to
====> n
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  you, and it fits in with the World Laboratory philosophy and
  also  with  the commitment  you  have  from Fleischmann  and
  Jones, is  that you come  to Utah,  where we can  visit both
  laboratories  on April  12 (or  April 12  and 13).   Clearly
  Ziegler  and I  can do  that more  readily than  Sicily, and
  Barnes is willing as well.

  I send this by FAX now (at  00:16 New York time) so that you
  will be able to think about it.  I will follow with a FAX at
  09:00  New  York  time giving  names,  addresses,  telephone
  numbers and  FAX numbers for  key people with whom  you will
  want to communicate.

  Unfortunately, I leave at noon Wednesday 04/05 for a trip to
  Boston and Colorado--  back late Friday night--  so you will
  have to  make further  arrangements without  my help.  I do
  expect to look  in on my computer mail each  evening, and to


====>
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                                                       PAGE 2

  talk with my office several times  a day, but I will be hard
  to reach.

  Sincerely yours,


  Richard L. Garwin

  cc:
     C. Barnes, via FAX to CalTech 9 (818) 795-1547
====>
                                                            X E D I T  1 File
                                                                 RECV

 095AZ    FAX      A1  V 80  Trunc=80 Size=133 Line=93 Col=1 Alt=0
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C. Barnes, via FAX to CalTech 9 (818) 795-1547
T.D. Lee, via FAX to Columbia Physics 9 (212) ???-????
✓ A. Zichichi, via FAX to CERN 9-011 (41-22) 82-7774
✓ J.R. Ziegler, IBM (via VNET)

RLG:rlg:095%AZ:040589..AZ

To: RLG2(YKTVMX)
From: ZIEGLER(YKTVMZ) 89/04/04 16:00:48

Will I get an invitation to the Italian one-day workshop, or should I just go ahead and get tickets? The round trip to Rome is $1600 ($2400 business class) if the trip is next week. Where in Sicily will it be?

Jim Ziegler

* * *

TECHNOLOGY

A Fusion Discovery Tentatively Confirmed

By DAVID STIPP
Staff Reporter of THE WALL STREET JOURNAL

Scientists at Brookhaven National Laboratory said they have tentatively confirmed the discovery of room-temperature fusion by Brigham Young University scientists.

"We're not absolutely certain, but we have detected neutrons [produced by fusion reactions] that are consistent with the Brigham Young result," said Kelvin Lynn, a researcher at Brookhaven, a federal government-supported lab in Upton, N.Y. The neutrons were produced in a batterylike device similar to ones employed in so-called cold fusion experiments by two Utah teams, whose reports have sparked worldwide fusion fervor.

The Brookhaven finding marks the first U.S. report supporting results obtained by the team at Brigham Young, in Provo, Utah, which is led by Steven Earl Jones, a physicist. While important to scientists, the preliminary confirmation doesn't necessarily have commercial implications—the fusion reactions involved produce far less energy than they consume, hence are of doubtful value for power generation.

Scientists still haven't reported replication of a more controversial, separate experiment by a University of Utah chemist, B. Stanley Pons, and his British colleague, Martin Fleischmann. Their claims, while considered far-fetched by many scientists, have sparked excitement, because the results indicate cold fusion can produce large amounts of energy. If the finding is confirmed, "It would be the most significant discovery since fire," said Philip Ross, a researcher at Lawrence Berkeley Laboratory, a federally supported facility that is associated with the University of California at Berkeley.

Fusion, the combining of the nuclei of hydrogen atoms to release energy, occurs at millions of degrees in hydrogen-bomb explosions and stars. Scientists have sought for decades to harness it for power generation. Two weeks ago, the University of Utah researchers reported they had generated copious quantities of heat and other signs of fusion in rods of the metallic element palladium, laden with deuterium, a form of hydrogen. Brigham Young's Mr. Jones last week reported that his team, using similar apparatus, independently found signs of fusion on a much smaller scale than that claimed by Messrs. Pons and Fleischmann.

Dozens of scientific teams now have set up such devices and already are developing theories on how they work. Researchers said the deuterium nuclei, consisting of a proton and a neutron, are held in close proximity by atoms in the crystal lattice of the palladium. This apparently sets the stage for their fusion through a phenomenon known as tunneling, in which some of the mutually repelling nuclei cross their barrier of repulsion to combine.

But tunneling requires the positively charged nuclei to move far closer than the distances separating them when trapped in the palladium lattice of atoms. Just what squeezes them close enough for fusion to occur—especially on the scale posited by the University of Utah scientists—is one of the central mysteries of the unfolding fusion story.

Philip Morrison, a physicist at the Massachusetts Institute of Technology, said the University of Utah scientists might have discovered "a new channel for nuclear reactions" that differs somewhat from previously observed fusion. One possibility, he said, is that many atoms in the palladium crystals jointly contribute energy to short-lived, electrically charged "quasi-particles." Such transient particles might interact with deuterium nuclei and push them close enough for tunneling and subsequent fusion. Scientists have proposed similar ideas to explain the Brigham Young report.

Mr. Morrison said that roughly analogous ideas, involving quasi-particles and crystal lattices, have been proposed to explain new superconductors, which are materials that conduct electricity without resistance. Indeed, some scientists already are devising theories that link the two phenomena.

Still, many scientists remain skeptical about the University of Utah report, because it didn't show neutron emissions that should have accompanied the fusion reaction with deuterium. Brookhaven's Mr. Lynn, however, said that his team is investigating other fusion reactions that may produce fewer neutrons than the fusing of deuterium nuclei do. His team, among others, suspects that lithium—a metal that is one of the ingredients of the electrolyte in the Pons-Fleischmann device—might be involved in such "exotic" fusion reactions.

Other scientists believe fusion might not be involved at all. "Who knows?" said Sam Hokin, a fusion researcher at the University of Wisconsin. "Maybe Pons and Fleischmann have invented the world's most interesting battery" that works without fusion.

In any case, scientists said, the same reaction might be possible in materials less expensive than palladium—the cost of which, at first glance, would be a major stumbling block to commercial applications of cold fusion.

United Technologies Unit's Job

HARTFORD, Conn.—Sikorsky Aircraft, a subsidiary of **United Technologies Corp.**, said it received a $54 million order from the Royal Hong Kong Auxiliary Air Force for five general-purpose helicopters and three search-and-rescue helicopters, with options for four more general-purpose helicopters.

Tandy Corp.

March Sales Rose as Results From Radio Shack Rebound

Tandy Corp., Fort Worth, Texas, said its Radio Shack sales rebounded some during March, and sales for the month rose 12% to $324.8 million from $289.8 million for March 1988.

Sales at U.S. stores open for more than a year increased 6% during the month, a sign that sales had picked up at the company's stores, a Tandy spokesman said. However, the spokesman said the company doesn't have enough information yet to know what products were hot-sellers. Tandy's same-store sales grew only 1% in February.

For the fiscal third quarter, ended March 31, the computer and electronics retailer said its sales increased 12%, to $944 million from $839.1 million last year. Third-quarter results are expected to be released later this month.

Tandy, in contrast to its usual practice, didn't release its U.S. retail sales for the month, saying those sales figures have been distorted by its February decision to transform about 60 Radio Shack Computer Stores into showrooms for its Grid computer unit.

In New York Stock Exchange composite trading yesterday, Tandy's stock closed at $41.125, down 75 cents.

Masstor, IBM in License Pact

SANTA CLARA, Calif.—**Masstor Systems Corp.** said it signed an agreement with **International Business Machines Corp.** to license technology and buy equipment for the manufacture of its own mass storage system cartridges. Details of the agreement weren't disclosed.

Masstor, a maker of mass storage systems for large-scale data processing, said making its own cartridges will boost control over the quality and availability of its products.

WHAT'S NEW, Friday, 31 March 1989 Washington, DC

1. SO FAR SCIENTISTS HAVE FOUND MORE CONFUSION THAN COLD FUSION. Researchers at several of the most prestigious university and industrial laboratories in the nation have so far been unable to reproduce the Utah results. But the Utah researchers, Pons and Fleischmann, now say that the experiment must be run for some ten days before fusion starts. From the few details in TV interviews and press reports, their claim is that deuterium nuclei can be so closely confined in a palladium cathode during electrolysis that fusion occurs by tunneling through the coulomb barrier, producing a net gain in energy of 400%. The tunneling probability is finite, of course, but even in the sun the probability is small. On the other hand, Edward Teller has endorsed the Utah claims.

2. A GROUP AT BRIGHAM YOUNG ALSO REPORTS COLD FUSION OF DEUTERIUM in metal lattices, but makes no claim of energy gain. The group is led by S.E. Jones, who demonstrated muon-catalysed fusion three years ago. They report observing neutron production during electrolytic infusion of deuterons into palladium and titanium.

3. JAMES C. FLETCHER WILL HEAD THE "COLD FUSION" EFFORT AT UTAH. The retiring NASA Administrator, who served as President of the University of Utah in the late sixties, will apparently have $5M to get things going while he negotiates with the more than 200 private companies that don't want to be left on the platform when the gravy train pulls out. The University says it will only sign with companies that agree to base some of their effort in Utah.

4. ADM. R.H. TRULY IS SAID TO BE THE TOP CANDIDATE TO HEAD NASA, replacing Fletcher who announced that he is stepping down as NASA Administrator on April 8, whether the Bush Administration has found a replacement or not. But Admiral Truly faces an obstacle that could be as hard to penetrate as the fusion barrier. The National Aeronautics and Space Act of 1958 states that the Administrator "...shall be appointed from civilian life by the President with the consent of the Senate." Unless Congress enacts new legislation, this would require Truly to resign from the Navy and give up his benefits, which he is reportedly reluctant to do. Even though Truly is highly regarded on the Hill for his role in rebuilding the shuttle program, members of Congress who oppose any further militarization of NASA will be reluctant to change the law. Sununu seems undaunted by the legal obstacle. Others being considered for the job include General Abrahamson, the former SDI chief, and General Lew Allen, the Director of JPL.

5. GEORGE B. RATHMAN IS ON THE SHORT LIST FOR SCIENCE ADVISOR. He is the Chairman of Amgen Inc., a California biotechnology company he founded in 1980. He received his PhD in Physical Chemistry from Princeton in 1952. He served as Chairman of the Industrial Biotechnology Association from 1986-88, but he has no Washington experience and is not widely known in the scientific community.

Robert L. Park (202) 232-0189 The American Physical Society

D.R.O. Morrison
CERN

Draft 2 (4 April) with new information, additions, corrections..

31 March 1989.

PHYSICS NEWS - COLD FUSION?

Dear E632 and WA84 Colleagues,

There have been many reports in the papers that Prof. Fleischmann of Southampton and Dr. Pons of Utah have evidence for cold fusion of deuterium by electrochemistry. This afternoon Prof. Fleischmann gave a seminar in CERN. Because of the many media reports, the auditorium was crowded and although I arrived 20 minutes early, I had to sit on the steps. As I have given several lectures on Wrong Results in Physics, I went to this and also to the press conference afterwards - especially as the news reports had been very hard to understand scientifically, but if true, this could have a major impact on the world economy.

Martin Fleischmann had a reputation as a major expert in his subject. As his talk developed, it became clear that he was a first class scientist and it seems to me that he has made a major breakthrough, though what the fundamentals processes are is not yet fully understood.

Let me try and explain what I think I learnt (I talked to him for a while afterwards, so it may not be too bad).

Basically the catalyst used, palladium Pd, is a face-centred crystal. It can absorb a certain amount of hydrogen. If an electrical potential is applied, then over a period of time it can absorb a great deal. For F & P, they reached 0.6 atoms of deuterium per atom of Palladium after three months.

They made tests with four rods each of 10 cm length and of diameters 0.1, 0.2, 0.4 and 0.8 cm. They only have good measurements for the first three as the fourth one was said to have "died" (this was indicated to be something chemical and to have nothing to do with fusion). They also tried a 1 x 1 x 1 cm3 cube but one morning when they came in they found that it had melted and the fume cupboard was starting to smoulder!

They made calorimetric measurements and found that they were getting more heat out than they had put in and this effect increased with the diameter of the rod. It seems to be a volume effect and not a surface effect. The excess heat is about 5 megajoules per cm3 which is about 100 times greater than any known chemical process.

A second measurement was by putting a NaI crystal close when they recorded gammas. The energy spectrum of the gammas was sharply peaked between 2000 and 2400 which is characteristic of the (n,gamma) reaction on hydrogen. This could be explained as the neutrons interacting in the water bath round the experiment.

Thirdly they observed tritium production and measured and found a "characteristic" spectrum (I did not understand this fully, partly as he had an incomplete scale on the graph, but see later).

Fourthly they looked for neutrons using a polythene sphere filled with BF3. The count was three times background. In 50 hours they counted 40 000 neutrons. However there is a point that is a stumbling block for particle physicists - if you take the rate of release of heat, then there should be 10 E 13 or 14 neutrons - a huge discrepancy. He does not have the equipment to measure the neutron spectrum - the neutrons have to pass through the surrounding water bath which tends to thermalise them.

A conclusion that can be drawn from Fleischmann's talk is that the heating is not due to the reactions

$$^{2}D + {}^{2}D \longrightarrow {}^{3}He + n \qquad (1)$$

or

$$^{2}D + {}^{3}T \longrightarrow {}^{4}He + n \qquad (2)$$

which are the ones that spring to mind.

He gave a table of the excess enthalpy in the Pd rod cathodes expressed as a percentage of breakeven values;

0.1 cm	81%
0.2	189%
0.4	839%

He opened his talk with a basic discussion of electrochemistry.

$$D_2O + e^- \longleftrightarrow D(\text{absorber}) + OD^-$$

$$D(\text{absorber}) \longleftrightarrow D(\text{lattice})$$

$$D(\text{absorber}) + D_2O + e^- \longleftrightarrow D_2 + OD^-$$

With the applied field the D can go over the potential barrier by applying a Potl. Difference at the interface. The result is that inside the Pd there can be many collisions without repulsion. Effectively there is a PD of 0.8 eV which can translate into a compression of 10 E 27 atmos. i.e. it would require this enormous pressure to achieve the same PD. Thus electrochemistry is high energy chemistry! The D is in a sea of high electron density. The structural or coherent strength of the Pd is 4000 atm. Thus it is a very strange kind of Quantum Mechanics (his phrase).

I have to go to collect my daughter at the airport, but will try and continue later.

1 April 1989.
(despite the date, it is serious!)

Re-reading what I wrote yesterday. I realise that I have been trying to explain simply. The actual talk contained some more details and two tables of results that I had only time to copy down partially. There was a fuller discussion of electrochemistry.

The question now is what is happening. The observations are of a source of heat, of emision of tritium, gammas and of neutrons, but the number of neutrons are many orders of magnitude less than would be expected if the heat produced came from reactions producing neutrons. Fleischmann talks as if you have to modify quantum mechanics - this I do not believe - we have to apply it differently.

An additional piece of information that he gave at his press conference but not at his seminar, was that the particle emission was not uniform but had fluctuations which were much larger than statistical - this I think is a very important piece of information.

There are a lot of different theories being discussed. The following comments should be considered private, qualitative and not necessarily correct.

The catalyst, palladium works by accepting an incredible number of deuterium nuclei in the spaces of its face-centred cubic lattice. The distance between each deuterium nucleus is therefore reduced. This was first demonstrated by the observation of muon-induced catalysis where in deuterium, the electron is replaced by a muon. As the muon is some 200 times heavier, the proton and neutron are pulled closer together so that the probability of fusion is greatly increased - by many orders of magnitude. Now there are two suggestions;

1. Since the deuterium nuclei are in a very dense electron field, it may be that the electrons have an effective mass much greater than normal and this increases the probabilty of the nuclei tunnelling through the barrier.
2. the applied potential difference drives more and more deuterium nuclei into the spaces between the palladium atoms so that the separation of the nuclei decreases so that the probability of fusion increases dramatically.

Personally I have a preference for the second approach, but it is always possible that both are applicable.

Instead of saying that there is a discrepancy between the number of neutrons produced and the heat produced, perhaps we should assume that all the results are correct and that the reactions ocurring are different. Maybe the dominant reaction is fusion, $D + D \longrightarrow {}^4He$, but we need something else to share the energy and momentum produced - this could be the close neighbouring structure of the lattice. Thus the dominant reaction is to produce heat! Of course other reactions will also occur which is why there is an observation of tritium and one would expect some production of ${}^3He$ and

4He and neutrons and gammas. If this were true, and again this is mainly a suggestion which needs experimental confirmation, then this would have tremendous social effects as we would have a simple source of energy without the particulate matter, sulphur and other gasses from coal and oil fired power stations that are killing so many today. Also the radiation danger would be very much less than with nuclear reactors (sell your coal and oil shares if you have any!)

In answer to a question, Fleischmann said that they had tried to look at 3He and 4He production and ratio, but the experiment is difficult for them and they prefer to leave that for experts who have the equipment - for they have been using their own money for 5 years.

Before the Seminar, things were rather disturbed with the media - lots of TV crews and flashes popping off. The Chairman, Carlo, asked them all to leave explaining this was a scientific meeting and he did not want questions on any other subject, but afterwards there would be a press conference. After some time the media left. At the end of Fleischmann's talk, the TV crews re-entered and had to be requested to leave again before the question period.

On the way to the press conference, Fleischmann was told that there had been a report on the radio that a group (at Columbia?) had confirmed his result. He said he had not heard this and during the Press Conference he continued to emphasise, in a very proper manner, that before leaping to conclusions, there should be further confirming evidence.

Fleischmann had described his other press conference in Utah as awful, but this one went well with Carlo a good Chairman - who was also asked questions. Fleischmann explained that the work was intentional and not an accident. He said that after verification, it might take 10 to 20 years to develop an economically viable system. Carlo was asked his opinion and said that "Dr. Fleischmann has planted a seed - will the seed grow up? I think yes" Fleischmann said that he believed in Karl Popper's philosophy - you cannot prove something right, you can only prove it wrong. "We have spent 5 years trying to prove ourselves wrong, now other people should try".

In explaining why they did it, "it was not to do an ego trip (though all scientists are on an ego trip to some extent), but to try and find a plentiful source of energy. We have a social conscience"

Question - "There was a sceptical atmosphere in the room, did you feel like a chemistry bull in an arena of physics toreadors?"

Answer - "Are people correct to be sceptical?, yes, it is correct to be sceptical. But it was not a bad atmosphere. Our experiment fits partly into accepted ideas but not entirely, therefore either experiment is wrong or we have extended the conceptions of possible fusion mechanisms".

Carlo was asked if he found the meeting strange - "No, I am at home in my own lab".

Question - "Do you think it is correct?". Answer(MF) - "I think it is correct, but others should show it is correct". (Note, this was typical of some of the questions where the journalist asked "for a good quote").

Carlo was asked if CERN should work on fusion. He replied " There are different science cultures. In an orchestra everyone tries to play his own instrument, and does not have other instruments. But we have quantum mechanics in common. We should do what we do best. But there is also cross-fertilisation between chemistry and nuclear physics" He also joked that this was the firsttime that a che

Question - "Any military applications?"

Answer(MF) - "There will always be some military application of anything, but we do not know of any such thing"

Question - " You said you did not have enough money, have you been offered money since your press conference last week"?

Answer - "Up to now have used our own money as we thought it unlikely to work, so there were some restrictions. Since then we have been approached with offers but as our capacity to spend money is limited, we have to plan carefully.

Question - "If it is fusion what will its effect be on other fusion research?"

Answer - " Glad you asked that. It would be a total disaster to cut back on other fusion research. Ours is small scale, theirs is large scale generation of electricity. It would be extremely foolish to cut back".

There was more, but I hope this gives the flavour - both Fleischmann and Carlo aquitted themselves very well and responsibly.

Friedrich Dydak had told me he had two papers confirming the F & P work and I could copy them. Later when I was returning them, Fleischmann came in for another TV interview and we talked while he was waiting for the lighting to be set up. He had not seen the papers, so I gave him copies. The main author was Stephen Jones who is at the BYU in Utah. We looked quickly at the papers - he was particularly interested in the dates on the papers.
I explained I was interested particularly for two reasons. Firstly as I was possibly the first to observe fusion in Europe - in the early sixties I was scanning bubble chamber film of deuterium and normally when there is the decay chain,

pion ---> muon ---> electron

the muon always has the same short range (if the pion is at rest). But one day I observed an extra long range for the muon. I spent some time measuring the curvature and angles of the tracks, but could not explain it. However someone told me that the Berkeley bubble chamber group had found it and it had been explained as the muon replacing an electron and causing fusion. At this Luis insisted that this should be treated as a secret, but quickly it was calculated that it had no military or economical value. So I left it and went on to new things(incidently the Scientific American article of July 1987 by Rafelski and Jones on Cold Nuclear Fusion says that this muon -induced fusion was first suggested by Frank and Sakharov in the late 1940's).

Secondly I said I had given several serious lectures on Wrong Results in Physics and found that they exhibited certain characteristics so that they could be recognised before they had been proved wrong - after the press reports I wondered if this was a case in point, but after I had heard his conference, I was inclined to believe that his results were correct. He did not seem to appreciate this too much, not unnaturally, but we continued talking and he told me some remarkable things. I mentioned that after the press conference, Dr. Wind was looking for him as he used to work in Utrecht on electrochemistry and had been able to insert 1000 hydrogen ions per atom of palladium catalyst. Dr. Fleischmann (who had attained 0.6 ions after 3 months) said he did not believe this number of 1000. It is possible that Dr. Wind was talking about the reduction in volume that can be obtained when hydrogen is absorbed in Palladium - this factor of 1000 in volume would then translate to 0.4 atoms of hydrogen per Pd atom in agreement with Dr.Fleischmann.

The two papers are;

1. "Observation of Cold Nuclear Fusion in Condensed Matter" by S.E. Jones and others of Brigham Young Univ. and J. Rafelski of Univ. of Arizona.
2. "Limits on Cold Fusion in Condensed Matter; a Parametric study" by J. Rafelski and others of Arizona and S.E. Jones of BYU.

The main point of the first paper is that they claim to have observed neutrons when there was low voltage electrolytic fusion of deuterons into metallic titanium or palladium. They believe this is from the reaction;

$$d + d \longrightarrow {}^3He(0.82\ MeV) + n(2.45\ MeV) \qquad (1)$$

The distribution of counts in different channels give a broad enhancement which the authors say corresponds to neutrons of 2.45 MeV. This looks convincing - just; it would be good to repeat this.

They say they have not yet(?!advertising?) obtained results regarding the parallel reaction;

$$d + d \longrightarrow p(3.02\ MeV) + t(1.01\ MeV) \qquad (3)$$

The electrolyte contains various mineral salts and they say that their evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode. "hydrogen bubbles were observed to form on the Pd foils only after several minutes of electrolysis, suggesting the rapid absorbtion of deuterons into the foil; oxygen bubbles formed at the anode immediately". The palladium pieces were 0.025cm thick and had the surfaces roughened or were mossy. They do not say that it took 3 months to get started by charging the deuterons into the palladium (private comment - this suggests to me that Fleischmann and Pons would have improved things if they had increased the surface to volume ratio of the catalyst and roughened its surface, but it is hard to be sure. However it does suggest that it is possible to charge the catalyst in much less than three months).

The experimental part of their paper gives an impression of haste, but there are a lot of other interesting things in their paper;

In a deuterium molecule the separation between the deuterons is 0.74 A and the d-d fusion rate is very slow about 10 E -74 per D2 molecule per sec (calculated in an interesting paper by Van Siclen, C.D. and Jones, S.E., Journal of Physics G Nucl. Phys. 12 (1986) 213 - here they state that the fusion rates for reactions (1) and (3) are nearly equal over the range 10 to 30 KeV. They also discuss whether piezonuclear fusion - i.e. by pressure - within the liquid metallic hydrogen core of Jupiter could account for the fact that the planet radiates 1.5 times as much heat as it receives from the sun. However they concluded that this process was many orders of magnitude too small to be a significant energy source - this is where the idea of Fleischmann and Pons of using electrolytic catalysis is so important). However in muon-induced catalysis the internuclear separation is reduced by about the ratio of the muon to the electron masses (200) resulting in the fusion rate increasing by an enormous factor, 80 orders of magnitude! In the second paper this variation of fusion rate as a function of the distance is quantified. This made me think of the observation by Fleischmann that they had observed large fluctuations in the signals - for the number of deuterons in a space in the lattice of Palladium is discrete and given by Poisson statistics hence the distance between the deuterons will vary appreciably - this and other factors(roughness of surface) could cause there to be local spots hot in space and time, since the fusion rate varies so violently with distance. In addition to the reactions (1) and (3), there can occur the reaction on tritium that will exist to some varable extent,

2D + 3T ---> 4He + n (2)

Although there is less tritium than deuterium, this reaction has a much higher cross section - so that this reaction (2) could also help fluctuations (but these comments on fluctations are my own, so treat them with appropriate caution).

Paper (1) also has an interesting chapter on Geophysical considerations (or the Hawaii effect). Sea water contains about one part in 7000 of deuterium. By subduction water is carried down to the earth's mantle where it might undergo fusion via the reaction;

p + d ---> 3He + gamma(5.4 MeV) (4)

under the extreme pressure and temperature there. Calculations are done which indicate that a substantial contribution to the heat flux through the crust could come from cold fusion. This heat could also help to explain the localised heat of volcanism at subduction zones. They quote that the 3He to 4He ratio is high in rocks, liquids and gases from volcanoes. Further they then predict that tritium will be produced from d + d fusion and since tritium is relatively short-lived(12 years half-life), observation of tritium would suggest a geologically recent process. On the Mauna Loa mountain on Hawaii, tritium was monitored from 1971 to 1977 and a correlation is shown in the paper between the tritium level and volcanic activity. This is very striking for the 1972 Mauna Ulu eruption but later eruption signals were partly confused by atomic bomb tests. They estimate that in the Mauna Ulu eruption 100 curies of tritium was released per day for 30 days!

In paper (1), it is also reported that after diamonds are sliced with a laser, the concentration of 4He and 3He has been measured - it is reported that the 4He is distributed uniformly while the 3He is concentrated in spots suggesting cold fusion reactions. Similar anomalies have been reported in metal foils.

The authors also calculate that the excess heat from Jupiter could be accounted for from cold fusion in the core consisting of metallic hydrogen plus iron silicate.

The second paper calculates the cold fusion rate of d-d as a function of 1 - relative energy, 2 - separation of two hydrogen nuclei in a sphere, 3 - the effective electron mass, 4 - the effective electron charge. They do not consider the effects of the lattice of a catalyst as do Fleischmann and Pons.

It is probable that some readers will be thinking that this letter has wandered off strict physics news. They are right. It is intentional as I feel this subject will become so important to society that we must consider the broader implications as well as the scientific ones. Looking into a cloudy crystal ball, it is not impossible to foresee the situation that the experiments are so easy that schools will be doing them, that many new companies will start up, most(not all) will fail and the present big power companies will be running down their oil and coal power stations while they are building deuterium separation plants and new power plants based on cold fusion. No new nuclear power stations will be built except for military needs. There will be very little if any research on high temperature(plasma) fusion. Petrol will probably still be used for cars and planes. Overall pollution will start to be less. Ecologists will be talking about the contamination from radioactive tritium and asking about the effect of this tritium on the ozone layer. On the other hand there could be major practical problems, e.g. the fusion could perhaps destroy the lattice structure of the catalyst before an efficient amount of heat has been extracted (one needs to understand and quantify why the 1 cm3 cube melted and why the 0.8 cm diameter rod went "dead"). Clearly many experiments, often simple, are waiting to be done

CONCLUSIONS

It is known(from muon cataysis) that if two nuclei of deuterium or tritium are held close together then they can fuse releasing energy. Fleischmann and Pons thought of achieving this by using electrolysis to insert deuterium nuclei inside a palladium catalyst. They observed production of more heat than they put in. They also observed tritium production, gammas of an energy consistent with neutrons interacting with the surrounding water bath, and neutrons directly. They thus conclude they have observed fusion of heavy hydrogen producing energy, i.e. cold fusion. A paper by Jones et al. reports on the operation of similar electrolytic cells with observation of neutrons with an energy spectrum consistent with that expected from deuterium fusion. They also describe interesting though rather anecdotal evidence for fusion in volcanoes, Jupiter, diamonds and metal foils. The theory, while not fully developed, suggests that the deuterium nuclei inside the lattice of the catalyst, are held so closely together that the probability of fusion(the tunneling effect) is dramatically increased by many orders of magnitude. If there do not turn out to be major practical problems, it may be expected that this will cause major changes in the energy industry and major social, economic and hence political changes.

Douglas R. O. Morrison.

ADDITION

4 April 1989.

The problem is to find an explanation for all the data, or alternatively most of the data.

The biggest problem is the discrepancy between the heat produced and the rate of neutron production in the reaction

$$d + d \longrightarrow {}^{3}He + n \qquad (1)$$

Occurring with about equal cross section is the reaction

$$d + d \longrightarrow t + p \qquad (3)$$

The other energetically possible reaction is

$$d + d \longrightarrow {}^{4}He$$

but this needs something else to carry off the energy - it could be a gamma but the cross section for this reaction is much less than for (1) or (3). The suggestion has been made that it could be the lattice of the palladium

$$d + d \longrightarrow {}^{4}He + L \qquad (5)$$

where L is the Lattice.

This sounds attractive as the ratio of the cross section for reaction (5) to reactions (1) and (3) is not known.

In discussing with John Ellis this morning he suggested a three-body reaction d + d + L ---> 4He + L* (6)

where L* would be an excited state of the Lattice.

However the energy released is about 19 MeV and this seems too much for the lattice which normals measures its excited states in eV. However if an entire region of the lattice were to move essentially coherently, say a few 1000 atoms, then MeV energies might be obtained. However there is the problem of timing - the nuclear reaction takes place in a much shorter time than the period of oscillation of a lattice.

So while reactions (1) and (3) probably do occur infrequently, we still need a reaction mechanism which is dominant and gives out most of the heat.

This afternoon I heard from two sources that Fleischmann and Pons used Lithium salts in their electrolytic solution! If the electrolysis were then to drive the lithium into the Palladium together with the deuterium then it would be would be possible to have the reaction

d + 6Li ---> 4He + 4He (7)

This is beautiful as it would explain how one gets energy(heat) but with fewer neutrons.

Looking again at the paper of Jones et al., they say they also used lithium salts! Their actual wording is of interest; "we developed the following (unoptimised) prescription for the electrolytic cells. The electrolyte is a mixture of about 160 g of deuterium oxide(D2O) plus various metal salts in about 0.2 g amounts each: FeSO4.7H2O, NiCl2.6H2O, PdCl2, CaCo3, Li2So4.H2O NaSO4.10H2O, CaH4(PO4)2.H2O, TiOSO4.H2SO4.8H2O, and a very small amount of AuCN. (Our evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode)".

Thus the experimental results can possibly be explained if the deuteron - lithium reaction (7) is dominant and the d - d reactions (1) and(3) occur but at a much lower rate.

Thus the ratio of heat to neutrons could be varied by varying the electrolyte composition.

In the paper of Jones et al. that records anomalies in volcano gases, Jupiter energy balance and 3He to 4He ratios in diamonds and metal foils, these effects could possibly be explained as resulting from different conditions and elements in the "electrolytic cell".

It should be noted that if (7) is the dominant reaction, then most of the energy will be emitted as Helium-4 nuclei and these should be searched for. Also these Helium-4 nuclei will cause severe damage to the Palladium rods which could also be studied. Again this damage could be important in constructing a power plant which is economic.

Douglas

4 April 1989.

COLD FUSION NEWS No 3.

Dear E632 and WA84 Collaborators,

Finally managed to get Martin Fleischmann on the phone at the new number and got answers to my many questions.

Firstly, yes, they did have Lithium in their electrolyte. He seemed a little surprised that a possible way of explaining the discrepancy between the large heat output and the low yield of neutrons, would be to assume that the lithium-deuterium fusion reaction

$$2D + 6Li \longrightarrow 4He + 4He \tag{1}$$

could be the dominant one while the deuterium fusion reactions

$$2D + 2D \longrightarrow 3He + n \tag{2}$$

$$2D + 2D \longrightarrow t + p \tag{3}$$

were present but much reduced.

He explained that they take samples of the electrolyte throughout the run and measure the tritium content. They find that the tritium level initially drops slightly and he interpreted this as the tritium undergoing fusion preferentially first via the reaction

$$2D + t \longrightarrow 4He + n \tag{4}$$

as the cross section is larger for reaction (4) than for reactions (2) and (3). The tritium level then starts to rise and goes well above the initial level.

The Lithium was not used for reasons of possible fusion but because it was light and because it was well known to help to keep the palladium together after lengthy catalysis.

The fact that the 0.8 cm diameter rod "died", was not something that worried him as apparently it is only moderately rare for palladium to be transferred back from the anode to the cathode. This rod gave out no measurable amount of heat, less than 0.1 mwatt.

The spectrum of gamma rays looked a little unusual with only a nice peak about 2.2 MeV, but he explained that there was some correction for Compton scattering.

The spectrum of electrons from tritium that I found difficult to understand, was not a Kurie plot but was a simple liquid scintillator.

He said that the heat output of 5 megajoules per cm3 was for a period of 100 hours, which was equal to about 10 watts per cm3.

In the last few days there have been reports in the media that physicists at the university Lajos Kossuth at Debrecen in Hungary had repeated the Fleishmann - Pons experiment - he said he knew them and felt they were good workers in this field.

They(F & P) have a number of experiments under way but these will take time.

5 April 1989.

Yesterday there was the hope of explaining the discrepancy of the observation of large amounts of heat but very few neutrons, by invoking the possible fusion of deuterium with 6Li to give two alpha particles, i.e. heat without neutrons. However last night I realised that if 6Li can fuse, why not 7Li? and calculations show the reaction is also exothermic

$$d + 6Li \longrightarrow 4He + 4He \quad + 22.4\ MeV \tag{1}$$

$$d + 7Li \longrightarrow 4He + 4\ He + n \quad + 12.9\ MeV \tag{2}$$

If one reaction takes place why not the other (note, cannot get 5He)?

Further natural lithium is 7.5% of 6Li and 92.5% of Li (and commercially available lithium is often seriously depleted in 6Li for military reasons) So that this also argues against reaction (1) being a solution of the discrepancy.

Hence the main mystery remains;

1. Fleischmann and Pons measure a heat output of about 10 watts per cm3
2. They measure a neutron production rate which assuming the reaction

$$d + d \longrightarrow 3He + n \quad + 4.08\ MeV \tag{3}$$

would alone give a rate of heat production many orders of magnitude

lower than observed.
3. They observe tritium production
4. They observe gammas of an energy which corresponds to neutrons interacting in the water bath
5. Jones et al. measure neutrons of about 2.4 MeV as expected from reaction (1) but at a rate that would give a heat output many orders of magnitude lower than F & P observe.
6. A group from Birmingham working with Rutherford lab people at RAL, are reported to have observed neutrons
7. The Hungarian group has claimed to observe fusion but so far have no information on what means of detection they used.

It may be judged that the idea that electrolytic catalysis can cause fusion to occur is almost confirmed experimentally.

There is only one result which suggests that the energy output is big enough to be of economic interest.

In discussing this with Martin Fleischmann this afternoon, he said that the experiment to measure the heat produced is not basically difficult, but it is very necessary to be careful. Thus he hopes that some other independent group repeats their experiment(they are doing it themselves, but independence is better, he rightly emphasises).

In his lecture he had said that an experiment takes months to perform, however recent experiments have been performed in days. He explained that to charge with deuterium a one millimeter diameter rod of Palladium takes about 2 days, a 2 mm rod 8 days, a 4 mm rod a month and an 8 mm rod about 4 months.

Hence after a fast qualitative result, the variation of measurements with rod diameter and other variables to obtain quantitative analysis can be long.

Thus it would appear that neutrons can be observed fairly quickly, but to do the more extensive experiment with calorimetry, will take longer. These are the crucial experiments that one now awaits to see if electrolytic fusion is economically viable or whether, like muon catalysis fusion, it occurs but is not useful in a practicable way.

Douglas.

Dear E632 and WA84 Collaborators, 9 April 1989.

COLD FUSION NEWS No 4

ABSTRACT

Copies of the Fleischmann and Pons paper have finally been received. General and detailed comments have been made, not all favourable plus some comments also on the Jones at al. paper. First reports are given on other labs' experiments. A new idea to explain the fusion results is discussed. Nature has written strongly on press releases and publication and suggests they might not accept the two groups' papers. This and other subjects are discussed.

1. DISCUSSION OF FLEISCHMANN AND PONS PAPER - GENERAL

The Fleischmann and Pons paper submitted on 11 March to the Journal of Electroanalytic Chemistry, has now begun to be distributed by telefax(on the first copy I received it was difficult to distinguish the letters and numbers as it seems to have passed through several telefaxes at least one of which was elderly - a later copy was readable). While a certain number of new things relative to the 31 March lecture at CERN, can be learnt(e.g. that results on rods with diameters up to 2 cm will be published elsewhere - Nature?), but the additional information is not great so that many questions asked are not answered. And indeed new questions are raised.
I would like to divide the comments into three parts; (1) evidence for fusion, (2) calorimetry, (3) relation between the measurements.

1.1 EVIDENCE FOR FUSION

(1.1.2) Increase in the tritium content. They take samples every two days of electrolyte and measure DTO, then replace this liquid with D2O. Prof. Fleischmann told me by phone that the tritium rate first decreased slightly and then increased steadily. He interpreted the initial decrease to dt fusion with the initial tritium in the heavy water - this has a much higher cross section than the dd fusion. It would be very interesting to see this result as a graph. It is to be hoped that the measurements were made at smaller intervals than two days as Prof. Fleischmann told me that the time to charge a 0.1 cm rod was two days and it was a 0.1 cm rod that was used for the tritium measurements. Blank experiments were made using platinium cathodes instead of palladium and indicated little accumulation of DTO. Measurements were only reported for the 0.1 cm rod. This is surprising since as the effect is said to vary with the volume, using the 0.4 cm rod would have increased the signal by a factor of 16.

(1.1.2) Observation of neutrons. They were measured with a 0.4 cm diameter rod at a current density of 64 mA per cm2 for 50 hours. The signal was 3 times the background. The counting efficiency for 2.5 MeV neutrons was 2.4 E -4 and there was a further reduction of 100 times due to unfavourable configuration (i.e. total efficiency was 2.4 E -6) After correction, the counting rate at the rod was 40 000 per second (which means that the actual counting rate was 0.1 neutrons per sec.) It is surprising that this important measurement was only done for 50 hours, only for one rod and only at one of the three current densities used.

(1.1.3) Observation of gammas. A peak is shown at 2.2 MeV, which is said to be consistent with gammas coming from the interaction

n(of 2.45 MeV) + p ---> d + gamma(of 2.5 MeV)

in the surrounding water bath giving evidence for production of neutrons of 2.45 MeV

The paper says the gamma measurement was with a 0.8 cm diameter rod but no results are given elsewhere for such a diameter and in his CERN lecture Prof. Fleischmann said the 0.8 cm diameter rod "died". Perhaps this is a misprint.

(1.1.4) It is stated that the intensity of the gamma spectrum is weak and is in agreement with the neutron flux calculated from measurements with a dosimeter. As elsewhere in the paper, no errors, either statistical or systematic, are given to justify this important statement.

It is stated that the calculated neutron flux of 40 000 per sec. for a 0.4 cm diameter rod, is in agreement with the tritium measurement of 12 000 per sec for a 0.1 cm rod "bearing in mind the difference in radii" but if the effect is a volume one, the radius factor is 16 and not 4.

It is curious that 3 different diameter rods were used for each of the 3 measurements instead of making each measurements with all three diameters

Also an important factor in the calorimetric measurements is the current density where excess heating measurements were made at 8, 64 and 512 mA per cm2. Large increases of the excess heating were reported as the current density was increased and as the rod diameter was increased. Hence one would have expected similar results for neutrons, gammas and tritium rates to be given for different current densities and different rod diameters, especially one would expect measurements to be made under the best conditions with maximum rod diameters and maximum current density. The current density is only given for the neutron measurement and not for the gamma or the tritium measurements.

There may be some questions of calibration and of null tests e.g. at Martin Fleischmann's talk last Friday, Carlo immediately asked if he had repeated the experiment with H2O instead of D2O - they had not; however in their paper they say they replaced the Palladium rod with platinium and observed no effect.

Overall, they seem to have concentrated on the calorimetric measurements that they knew best while the measurements of neutrons, gammas and tritium have been weak and reported too briefly.

1.2. CALORIMETRY

Dewars were fitted with resistance heaters for the determination of Newton's laws of cooling losses; temperatures were measured with calibrated thermistors. From the heat balance calculated, it is stated that the enthalpy generation can exceed 10 watts per cm3 of the palladium electrode. This was observed over experimental times in excess 120 hours.

No statistical or systematic errors are given. What to my mind is more serious is that no control experiment is reported. I would have been happier if they had put a heating element in a small hole in the Pd rod and applied heating of say 0.1, 1.0, 10, 100 watts and checked that their measurements and calculations gave the same wattage. Another result that one would have liked to see, was the variation of the wattage as a function of time. The point is that it takes an appreciable time to charge up the palladium rod with deuterium so that initially one would expect no heating and then the heating would build up to some plateau value.

Augustin McEvoy of EPF Lausanne in describing a physicists view of calorimetry said it was rather delicate. In the Fleischmann experiment there is no description available as to how is taken into consideration the hydrogen and oxygen gases that bubble off the electrode and which carry considerable energy and probably some water vapour.

It is reported that when "using D2O alone, a substantial portion of the cathode fused (melting point 1554 C), part of it vaporised and the cell and contents and part of the fume cupboard housing the experiment was destroyed". It would be good to have an explanation of this dramatic incident, which occurred during the night and only once so that it cannot be considered an experiment as it does not fulfil the requirement of reproduceability. In charging the palladium rod with deuterium, a considerable amount of work is done so that the rod contains appreciable energy(it is deformed). I tried asking Martin Fleischmann if it were possible that if there were an overnight fire in the fume cupboard, whether this energy could melt the rod but he discounted the suggestion.

1.3. RELATION BETWEEN FUSION AND CALORIMETRIC RESULTS

Fleischmann and Pons calculate that the "enthalpy generation can exceed 10 watts per cm3 of the palladium volume; this is maintained for experiment times in excess of 120 hours It is inconceivable that this could be due to anything but nuclear processes." No error, statistical or systematic is given. In his CERN lecture Prof. Fleischmann said any chemical process would have a energy rate 100 times less. However for the only nuclear processes measured, they say have energy releases which are seven to ten orders of magnitude less - it is very hard to believe that there could be other nuclear processes which are 7 to 10 orders more than one expects. Again they write "It is evident that the fusion reactions observed are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

The provisional conclusion may be that the results reported for the heat production have a different origin from the results reported for nuclear processes (n, t, and gammas).

The comments here have been given on first viewing of the paper and hence should be treated with caution.

2 DISCUSSION OF FLEISCHMANN AND PONS PAPER - DETAILED

Have received(via John Ellis) a note by Tom Walsh which gives detailed comments on the two experimental papers.

2.1 GAMMAS. The F & P paper shows a sharp gamma peak at 2.24 MeV. They claim this is from the reaction

n(2.45 MeV) + p ---> d + gamma (1)

and they expect the gamma to have an energy of 2.5 MeV. However Walsh calculates that the gamma energy should be 3.5 MeV (have just calculated that the Q-value of reaction (1) is 2.23 MeV). Walsh says that their peak demonstrates that it is produced by neutrons of less than 0.1 MeV - evidence against having been produced by fusion neutrons (he does not consider whether the neutrons could have been slowed before reaching the water bath - difficult to evaluate from the information available).

He says the average counting rate for the observed peak is 0.002 per sec. He reckons the background rate of neutrons of less than 0.1 MeV from cosmic rays is about one neutron per sec. for their 10 cm2 counter. He writes that the correct way to do this is to measure the photon spectrum with and without an active electrode.

2.2 NEUTRONS Again they should make their measurements with an active and an inactive electrode rather than going 50 metres away. He says they mesure 100 neutrons per sec, but he has perhaps omitted the counter efficiency of 2.4 E-4 so that I reckon they actually measure only 0.02 neutrons per sec. so will drop the remainder of his remarks on this subject.

2.3 TRITIUM Again he says one should measure with active and inactive electrodes - as measuring at a distance to establish background could be dangerous as there is so much tritium from bombs around.

3. COMMENTS ON JONES ET AL. PAPER

Tom Walsh says that it appears to be a carefully done experiment to establish the presence of 2.45 MeV neutrons from d + d ---> 3He + n. He comments that the neutron counter is rather poor in that it counts all neutrons above a certain energy (either 1 KeV or 0.5 MeV, he is not sure) and that better neutron counters exist.

He estimates the flux of cosmic neutrons of energy greater than 1 MeV is about one per second (the flux falls as 1/T where T is the kinetic energy) for their 100 cm2 detector. He says the measured rate is about 0.1 n per sec (I read it as 0.4) so that the background is large compared to the signal as can be seen from the figures.

Jones et al. quote the 2.45 MeV signal as being at the 5 sigma level,this being fitted over a width of 28 channels from inspection of the graph, however Walsh estimates that their resolution is about 50 to 100 channels and if they fitted with such a resolution the statistical significance would be less than 5 sigma.

Jones et al. write that the neutron counting rate often drops off after 8 hours and feel they can explain this. However Walsh wonders if the nearby Van de Graaf which is switched off during neutron measurements could be producing a daughter radionucleide with an 8 hour half-life.

About the observation of tritium from volcanic eruptions, Walsh comments that some tritium from bomb explosions will have been deposited by rain in the neighbourhood, seep down and some will be expelled by the volcanic eruption - he expects the effect to exist but the question is how much tritium comes from this effect.

Have just being discussing with Dr. Yves Declais of Annecy who is an expert on low energy neutron counters. Now the Jones et al. results show a very high baccground which almost hides the signal. Normally one would shield the counter with lead to reduce the gamma rays, then water to slow neutrons, next a boron shet to absorb thermal neutrons and finally for cosmic ray muons, a liquid scintillator counter should surround the neutron detector. This should allow a 2.45 MeV neutron signal to be seen clearly with little background due to gammas, low energy neutrons or muons.

4. NEW EXPERIMENTS REPORTED

Many experiments have been set up to check the results of Fleischmann & Pons and Jones et al. - at least 15 but probably more.

4.1 Brookhaven (was first told of a report in Thursday's Wall Street Journal by one of my daughters - no physicist has talked of this around here) which reported probably confirming fusion. Saturday's International Herald Tribune gave greater precision, a spokesman was quoted as saying "fusion events apparently have been seen, but not at a statistically significant level"! However see later under "Rumours", section 8.1.

4.2 A team at the Univ. of Debrecen in Hungary reported indications of fusion - no details are available.

4.3 At Harwell a team led by Prof David Williams has a large number(11) of cells operating. At an Institute of Physics meeting at Harwell on Thursday he gave a talk. After running 5 to 10 days they have not observed any effect. However I do not know the diameter of their electrodes and so do not know the time to charge them.

4.4 At EPF Lausanne, Prof Michael Grapzel and Dr. Cary Miller have been running an electrolytic cell for six days and are measuring the heat balance. So far they observed balance i.e. no effect. Asked the diameter of their rods - is 0.2 cm, so on Prof Fleischmann's estimate it should take 8 days to charge up(it is not clear to me why the charging time is proportional to the square of the diameter when the ions are said to be "highly mobile". One would expect the outer layers of the rod to be charged fairly quickly and start giving fusion well before the inner layers).

Note added 10/4/89; Have just spent some time with Dr. Miller. He explained in some detail the practical problems of the experiment. The calorimetric measurements are clearly much more difficult than one would imagine from reading the F and P paper and they require a great deal of care as it is rather easy to obtain a false result. Similarly to believe a result, one has to be able to study a great deal of detail and up to now this has not been available.

5. NEW IDEA TO EXPLAIN FUSION RESULTS

Prof Grapzel suggests that the fusion can be induced by cosmic ray muons which catalyse the d-d fusion reaction. He has asked an Irish colleague, Dr. O'Sullivan to calculate the rate, and thinks it could contribute to explain the Fleishmann and Pons results.

At first I was surprised and asked if all muons had been taken not only stopping ones but he checked that it was only stopping ones. Guiseppe Cocconi has now calculated that the flux of stopping muons near sea level is about 2 E-5 per gram per second. Discussing with Dr. McEvoy of Lausanne, they have a roughly similar estimate.

That muons could induce some fusion reactions in palladium is clear but the question is whether the rate was big enough to account for the two Utah results. It seemed to me that the best way to test this would be to vary the muon flux by puting the electrolytic cell in a muon beam and varying the flux of muons and measuring effects such as the number of neutrons emitted with a good selective neutron detector.

In searching for a suitable muon beam, I discussed with Dr. Niinikoski who is a member of the group doing experiments where polarised muons are implanted into metal samples and the muon spin precession in a magnetic field is studied. This yields information on where a mu+ lodges and how it moves. He said that if a first mu+ lodges in a lattice it creates a deformation. When a second mu+ diffuses along, it tends to lodge close to the first mu+. This I found very important as it would indicate that since D+ ions should be similar to mu+, it is to be expected that the separation distance between D+ ions would be small and hence fusion probabilities would be enhanced.

Combining this information, the separation between D+ ions would be reduced by the high density of D+ in the palladium, by the tendancy of the ions to seek neighbouring sites and by muon fusion catalysis. However further information indicated that this would only occur at high D+ densities. A preliminary conclusion is that one would need much more information before performing an experiment which introduced a muon beam into a palladium catalyst, though ultimately it could be of interest for scientific research reasons only.

An excellent introductory review of muon catalysis is that of Steven Jones (Nature 321 (1986) pages 127 to 133) while more recent work can be found in the Journal of Muon Catalysised Fusion which publishes papers of their annual conference(next is scheduled for June 1989 in Vienna). To make muon catalysised fusion economically interesting, each muon must catalyse many fusion reactions. Gajewski and Jones(MCF 2 (1988) 93) estimated this number, N(payoff), as about 1000 though it could be lower if muon beams with cheaper muons could be produced The original Berkeley bubble chamber work had liquid hydrogen and less than one fusion per muon. Since then remarkable progress has been made in understanding the importance of temperature and resonance effects. The result is that at Los Alamos an average of 150 fusions have been reached with dense deuterium-tritium mixtures(S. E. Jones et al. PRL 56 (1986) 588) and at SIN near Zurich rates of 124 +/- 100 have been observed (C.Petitjean, MCF 2 (1988) 37).

While d-t fusion occurs in 1 E-12 seconds and the formation of a dtmu molecule takes 1 E-8 seconds, which are much shorter than the 2 E-6 second muon lifetime, the problem is that the muon will tend to stick to the Helium molecule. For this reason one should not be optimistic about developing an economic process by passing negative muons into an electrolytic cell with a palladium electrode in a deuterium-tritium medium as these muons will tend to stick to the massive palladium molecule (note that the muons used in the spin experiments were positive). Dr McEvoy reckons that the flux of cosmic muons is such that to reproduce the neutron rate claimed by Fleischmann and Pons, each muon would have to catalyse several hundred fusions - for the reasons given above (sticking of muons in the Pd atom), this is most unlikely.

However the subject is of great interest and Fleischmann and Pons should be congratulated for opening up the subject of using electrolytic storing of hydrogen isotopes in catalysts.

6 WRONG RESULTS IN PHYSICS

In studying wrong results, I found 12 characteristics that helps one to recognise an experiment that is liable to be proved wrong. Most wrong results tend to fulfil about 6 to 8 of these characteristics. For example time-wise there are three phases;

phase 1 - the initial experiment is quickly confirmed by a few other experiments

phase 2 - an era of confusion, several experiments confirm the result while a roughly equal number do not find the effect phase 3 - all experiments do not find the effect; this happens like an avalanche (because those people who did find the effect feel happy and publish quickly while those who did not find the effect suspect they are missing some vital ingredient and so spend a long time checking, but once some others publish negative findings they quickly publish also)

Unfortunately the Fleischmann and Pons paper is slowly increasing the number of negative characteristics.

An LBL electrochemist is quoted as saying that "chemists looking at the reaction cannot explain it by chemical means, so they conclude that the process must be nuclear, while nuclear physicists say they cannot explain it by nuclear processes, and so think it must be a chemical reaction." There is a third hypothesis to be added - that the result is wrong.

7. PUBLICATION POLICY

In the 30 March edition of Nature there is an editorial and also a News item about the two Utah papers. The editorial is written in a general philosophical manner and emphasises the need for peer review, especially for unique or unusual approaches to problems. However the editorial notes that many journalists have learnt to be wary of unsubstaniated claims and to ask around before printing the story. The editorial is headlined"Reports that an account of cold nuclear fusion is soon to appear in this journal are premature". The News item describes some of the cold fusion reports and says the University of Utah has applied for a patent on the process. It also writes "phone calls to Nature's Washington office enquiring whether we had accepted or were about to publish one or more papers. (The answer is no)".

There are some interesting points about this. In working in particle physics one tends to think that what our community does is normal but talking with Prof. Fleischmann one was reminded otherwise. Many years ago the custom in particle physics was to write a paper, submit it for publication, wait for the referee's report, correct the paper, resubmit it and if it were accepted then distribute reprints. Prof. Fleischmann was astonished when I told him that for some time the habit was to write a paper and immediately distribute preprints widely at the same time as sending it to a journal for publication. Thus there is no peer review of the preprint, only of the published paper which sometimes has mainly archival value. I tried to encourage him to do the same with his fusion paper in view of the news stories. However he told me that in electrochemistry the older procedure was still in force.

But what about the press conference? Well those particle physicists who have become interested in Astrophysics find they have another and very useful technique of publishing. Observational results are sent by telex or telefax to the International Astronomical Union and these very brief communications are distributed by telegrams or by electronic mail to all subscribers. Thus when Supernova 1987A was first observed, everyone was alerted and people quickly directed their efforts to this new phenomenon while it was still there whereas if one had waited for a preprint or peer review, many useful observations would have been missed. Thus this is an example where the greater need of the community dominates over the advantages of peer review. If it were assumed that the Fleischmann and Pons result were correct, some justification for fast publication (e.g. a press conference) could be made.

8. OTHER ITEMS

8.1 Rumours are dangerous, sometimes false, sometimes true and preceding publication. Two examples - 1. An electronic mail message wrote that on Thursday it was said that both Brookhaven and Bell labs had retracted their confirmation of the cold fusion experiment(did not even know Bell labs were doing an experiment!). The message then says "this is at least a fourth order rumor". 2. On 31 March the radio reported that Columbia had also found cold fusion. Maurice Jacob thinks he understands this media story - Dr. Jones had just given a lecture at Columbia about his results obtained at BYU!

8.2 Have been given a graph of the price of Palladium - it has risen slowly over the past six months but since the last days of March has risen steeply and also the number of contracts is rising sharply! However the Journal De Geneve writes that this market is not for the man in the street.

8.3 The Observer of 9 April reports that the Utah legislature approved $5 million for research into the reported discovery of simple and cheap nuclear fusion by University of Utah scientists.

8.4 Have just received a preprint dated 7 April by S.E.Koonin and M. Nauenberg where they have calculated cold fusion rates in diatomic hydrogen molecules involving various isotopes. They find that the d-d fusion rate is 3 E-64 which is 10 orders of magnitude faster than the previous estimate of Van Siclen and Jones (a small change in the calculation makes a very big change in the result). They also find that the rate for p-d fusion is 1 E-55 which is faster because of enhanced tunneling with the lighter proton. However their calculation is for FREE diatomic molecules and hence does not of necessity apply to D+ in a lattice of palladium. It would be interesting to know if such a calculation has been done.

As considerable new information has come recently, it would be surprising if there were not some mistakes - please inform me and accept my apologies.

Douglas R. O. Morrison.

PS 10/4/89 Have just been informed from two sources that the Texas A and M have held a press conference to announce that they have found some agreement with Fleischmann and Pons by finding heat production. They do not commit themselves to saying it is fusion. Apparently they have some possibility of measuring neutrons and helium.

Are we in Phase 2 of section 6? i.e. con-fusion.

17.

Dear E632 and WA84 Collaborators

12 April 1989.

COLD FUSION NEWS No 5.

I have just had a long talk with Prof. David Williams of Harwell. He described the many and varied experiments and checks that they have been doing for some weeks. He is rather worried that there have been rumours abroad that they have found neutrons from the charged palladium rod - he would like to say very clearly that this is incorrect.

He says that they remember previous fusion claims and hence do not want to make any statement at this time. I suggested to him that in view of the intense interest that this subject had attracted recently, that there was a possible conflict between his scientific scruples of not making an important statement before all checks had been done and his social responsibility which would encourage an earlier report when checks have been done on most experiments though not all. So he suggested that one could say that they have not observed neutrons at the level that would be expected from the reports of Fleischmann and Pons and of Jones et al.

Douglas R. O. Morrison.

Dear E632 and WA84 Collaborators, 13 April 1989.

COLD FUSION NEWS No 6

In the history of cold fusion, we seem to have passed phase one where
(1) preprints of the original two Utah experiments have become available for study and are often considered to be hastily written accounts of work that has been done with few controls and is described with so few details that it is difficult to judge the truth or falsehood of the results
(2) press reports of experiments claiming to confirm these results but descriptions are not available to the scientific community.

Now we are in phase two where we do not need more press releases but experiments with good measuring devices and serious controls that would convince other scientists. In working on cold fusion, one is quickly aware of the great knowledge gap between electrochemists and particle physicists and also the different cultures and jargon. An ideal team would include both electrochemists and particle physicists.

A major criticism of the neutron measurements, particularly by Tom Walsh, is that the counting rates are close to the cosmic ray rate. If this background could be reduced by a few orders of magnitude, then any neutron signal would be unequivocally established. Such a neutron detector with a very low background has been developed by the groups of the Institut des Sciences Grenoble, College de France Paris, CPPM Marseille, Saclay, and LAPP Annecy at the Bugey Reactor centre near Lyon in the course of studies of neutrino oscillations where a low background has been found to be essential. It is always dangerous to say something is the best in the world, but this is one of the best.

Dr. Cary Miller has make an electrolytic cell at the EPF Lausanne with a palladium rod of 0.18 cm diameter and 5 cm length and has been running it for 8 days for calorimetric measurements. This morning we took it to Bugey (this is the first time I have had a experiment small enough to fit in my car and which was running) and installed it inside the shielding of the Bugey neutron counter. After a few hours running, already a neutron rate could be established substantially below the Jones at al. rate and several orders of magnitude below the Fleischmann and Pons rate and this is a very conservative estimate which will soon be improved. The shielding is such that the total background rate was about 1 E2 per hour and this was before discrimination between neutrons and other particles. When the veto against muons was removed (it is only on a few percent of the time), the background increased and indications of neutrons could be observed (stopping negative muons are known to produce neutrons via the decay electrons which interact with protons to give neutrons).

There is considerable space inside the shielding of this detector and the constructors (IN2P3 and CEA) invite any groups that have electrolytic cells wishing to study cold fusion to come to Bugey and make use of the detector. Please contact Dr. Yves Declais at LAPP Annecy, Email address;
LAPPVX::YD

Earlier versions of the detector and of the pulse shape discriminator are described in R. Aleksan et al. NIM A273 (1988) 303 and NIM A274 (1989) 203. The shielding has an outer layer of 10 cm of lead to reduce gammas, 25 cm of water to slow/stop neutrons, then 5 mm of a plastic containing boron to absorb the slow neutrons and on the inside 10 cm of liquid scintillator which counts muons mainly and is normally used as a veto (in our case we wish also to see if muons can cause more neutrons by muon fusion catalysis - Dr. Petitjean of SIN who is an expert on muon catalysis, has confirmed what was written in note No 4, that muons are expected to be unable to cause a series of fusion catalysis reactions as they will be captured by the heavy palladium ions and held strongly). Inside this large shielding box, there is the detector of 600 litres consisting of 98 cells which contain 6Li in the new liquid scintillator NE320. The slowing of the neutrons gives recoil protons and the signal amplitude is correlated to the neutron energy. When the neutron stops it reacts with 6Li to give tritium and 3He and the height of this signal is also measured. The distribution of the times between these two signals is used to determine the fraction of neutrons. Also the relation between the heights of the two pulses so gives a separation between protons and electrons - this method is called Pulse Shape Discrimination, PSD.

This Collaboration also offers space in the Frejus tunnel to any group who would like to make use of the low background level and large amount of space available there. Again please contact Dr. Declais.

Have received messages telling me that as well as Texas A and M confirming the calorimetric measurements of Fleischmann and Pons, Georgia Tech report a neutron flux 13 times higher than background - it would be good to wait and see more details.

In discussing this morning with Prof. Michael Gratzel and Dr. Cary Miller of the Ecole Polytechnique Federal of Lausanne, I learnt the importance of the footnote c to table 2 of Fleischmann and Pons's paper - these high values of the excess heat as % of the break-even, are not directly measured but assume a possible future scheme in which the deuterium gas released is not lost but recombines with the hydroxyl radical OD to give heavy water D2O.

Douglas R. O. Morrison

PS Have just heard that Prof Jones of Brigham Young University will lecture at CERN at 14.30 on Monday 17 April.

Dear E632 and WA84 Collaborators, 15 April 1989.

COLD FUSION NEWS No 7

There is an Informal Network of scientists who exchange information about their work and their ideas "in confidence". Including this the overall situation about cold fusion appears to be;

IN FAVOUR

1. The two original experiments, performed with Palladium (F & P) and with Titanium (Jones), gave two unusual results;
   1.A Production of excess heat(Fleischmann and Pons only)
   1.B Nuclear-type effects, production of tritium, gammas and neutrons
   These results have been critised in detail and are not consistent (the heat is about 10 orders of magnitude greater than would be expected if the origin were nuclear according to the results 1.B, also the neutron rates observed in the two Utah experiments disagree by several orders of magnitude (but see below).
2. There are several confirming experiments. One or two have been withdrawn since (Georgia Tech and possibly Brookhaven). However the main conclusions are known only from press conferences, except in one case where informal information casts serious doubts on the confirmation.

AGAINST

1. There are no published papers (or press conferences) saying that the experiments were repeated and the results claimed were not found, i.e. there are no null results.
2. Many groups are performing the experiments - today's Int. Herald Tribune says hundreds.
3. From the Informal Network I know that there are a number of groups that find no effect. For some of these groups I know enough details to believe that the work was done carefully and with good equipment.

DISCUSSION

1. For the majority of people, the case against is almost unknown, while the evidence in favour seems to be getting stronger and stronger (as shown by the smart money men who are pushing the price of palladium to ever higher values).
2. But the members of the Informal Network think the opposite, they are more and more convinced that there is no new source of heat and that if there are any nuclear effects, there are at a very much lower rate than published.
3. There have been great hopes that this cold fusion in an electrolytic cell could give almost limitless supplies of energy and that with little radiation or other pollution. Everyone would love it and greatly wishes it were true. But to their regret, those with access to the information of the Informal Network, believe it is not true.

WHY DO GROUPS NOT PUBLISH NEGATIVE RESULTS?

Would it not be so simple just for each experiment to publish so that everyone could judge?

Each group that separately does not find any effect, has two problems
(a) they ask themselves how can they be sure they themselves have not made a mistake somewhere that blocked them from finding the effect? - answer is that they can never be completely sure
(b) They ask if there is some secret part of the technique that Fleischmann and Pons and Jones et al. use that they have not talked about for some reason - if only they knew this they would find the effect.

Hence up to now no group has wanted to publish a null result.

There is possibly also a third reason for delaying announcing a negative result - since there has been so much excitement about the press announcements of a positive result and such very high hopes have been raised, that the first group to announce they did not see it would be subject to a tough media questioning "How can YOU be so sure that you have not missed it when several others have found it?"

The second worry (b) can perhaps be answered by noticing that other groups claim to have found the effects without asking the Utah groups for their secret.

So how could "no effect" results be presented? It needs either a strong group that has done many experiments and careful checks to publish alone or for several groups to present their results at the same time, e.g. at a conference. Who will be the first to say the Emperor has no clothes.

Douglas R. O. Morrison.

Dear E632 and WA84 Collaborators, 17 April 1989.

COLD FUSION NEWS NO. 8

Today Prof. Steven Jones of Brigham Young University visited CERN and gave a lecture then a few of us involved in experiments, talked with him in more detail afterwards. His talk was very cheerful and pleasant.

The first part of his talk was about muon-induced fusion, a subject in which he is a world expert and it is his group that has managed to have muons make 150 deuterium-tritium catalyses on the average before decaying or being stuck on a produced helium nucleus. He said that Van Siclen and he had calculated the fusion rate for a free deuterium molcule as

Lambda(f) = 1 E-74 per deuterium moleclue per sec.

which corresponds to one fusion per year in the galaxy!
[a very recent paper by Koonin and Nauenberg gives 1 E-64 - my afterthoughts are in square brackets]

In 1986 they proposed "piezoelectric fusion" where pressure could cause molecules of heavy hydrogen to fuse. He showed a very interesting map of the world with contours of heat flow in units of mW per m2. The greatest heat flow occurred far off the Pacific coast of the American continent, particularly off the coast of Peru. Other hot spots were roughly, Hawaii, the Red Sea, Iceland, North-east Italy [where the North African plate impinges on the European plates causing the rise of the Alps], etc. He then superimposed a contour map of regions of the earth where the ratio of 3He to 4He was much greater than normal - for example Hawaii was 21 times, 10 to 12 times in the Pacific hot spots, 12 x in the Red Sea region, 18 x near Iceland, 10 x in the N-E Italian region, The correlation was striking. Prof Jones explained this extra heating as possibly being caused by "piezoelectric fusion" of proton-deuteron molecules, produced in the subduction of the Pacific tectonic plate as it forces its way under the American land mass. For this he calculated the fusion rate to be

Lambda(f) = 1 E-24 fusions per deuteron per sec.

[in their paper they gave 1 E-18]. He quoted other examples of anomalous 3He to 4He ratios in metal foils.

Prof. Jones said their work in electrolytic cells started in May 1986 and as evidence of this showed pages of a log book (later Prof. Jones gave me a write-up of the history of their work which mentions that some pages of notes dated 7 April 1986 were notarised that day by a BYU attorney "showing the importance attached to these ideas by the physicists present". [some later parts of this long write-up are rather painful to read]).

He then described his experiments basically as in his preprint. He said that titanium was better than palladium - both had been used. He showed the calibration curve of the neutron counter for 5.2 MeV neutrons (not in preprint, it showed a broad peak near channel 270). Taking his Run 6, he said the shape of the observed peak near channel 100 is what would be expected from neutrons of 2.5 MeV. He gave numbers showing the rate of neutrons near 2.5 MeV was (6.2 +/- 1.3) E-4 while there was no effect on subtracting foreground from background for energies > 2.7 MeV (later there were serious questions on the errors from this reference region).

Run 6 which was the one of the 14 runs reported which had the most significant effect, used a titanium "sponge" of 3 grams - for it he derived a fusion production rate of 0.4 fusions per sec. giving a rate of cold fusion production rate of

Lambda(f) = 1 E-23 fusions/deuteron pair/sec

He showed the signals for the 14 runs (fig 4) and said they were uneasy at the way the signal varies so that they could not predict the neutron rate.

He explained that the unusual collection of salts used for the electrolyte was learnt by trial and was supposed to represent the mixture of salts available in geology [how could they make trials if the only positive results obtained were the 14 runs?].

The surface of the electrode was rough(later he gave us samples - it looked like a mixture of crystals and had sharp points) and was like denderites. They felt that the sharp points would have a stronger electric field and would "shield the Coulomb barrier". Work remains to be done to disentangle the features. When they ran with zero current, no effect was observed. When water was used instead of D2O, no effect was observed.

In his conclusions he said they had observed neutrons as a 5 standard deviation effect at a rate of 0.4 neutrons per sec. This corresponded to a heat production of 1 E-13 watts. He felt that this was new physics and could be a mono-energetic source of neutrons.

Carlo asked whether he thought that his experiment and Fleischmann and Pons's could agree? Prof. Jones said that they had never done calorimetry nor had they observed a rod melting. The two neutron results looked different and the peak shown by Fleischmann and Pons looked too narrow.

There were several questions about the significance of the 5 sigma claimed, in particular by Yves Declais and Charles Peyrou concerning the choice of control region, the scaling of the background and the apparent non-inclusion of the errors of the background. This would appear to reduce the statistical significance to much less than 5 sigma.

I asked then and later in private discussion, about the calibration with 2.9 MeV neutrons which was mentioned in the paper while only the more remote 5.2 MeV neutron calibration curve was shown - the 2.9 MeV has a curious shape with a sharp fall-off at channel 190 (corresponding to 2.9 MeV). But as the response is non-linear it is not really possible to check if the width of 28 channels claimed in the paper is correct or whether it is wider which would reduce the statistical significance. Overall the feeling was that if a rigourous analysis were made by physicists familiar with neutron counters, it could well turn out that the significance was considerably less than 5 sigma.

Bernard Hyams asked some interesting questions - as you have been running for three years, you must have done thousands of experiments (since they last about 8 hours each), so the probability of finding one series with a 5 sigma effect, is quite big. Prof Jones said this was based on a false premise as until December 1988, they only had a neutron flux monitor, after they had the good neutron counter. Bernard then asked if these were all the runs, and was told yes, except for one run of four days with two cells, but the cells soon turned off and the counts were not statistically above background [this troubled me as part of the syndrome of Wrong Results in Physics is that runs are discarded because they do not show the effect and therefore there must be something wrong with them. This reminded me of the 0.8 cm rod of Fleischmann and Pons which was declared "dead", but when I asked Prof. Fleischmann what his definition of "dead" was, he replied that it gave no heat, less than a milliwatt and so it must be "dead"].

Jean-Francois Cavaignac said that they had a carefully shielded neutron counter with very low background at Bugey and in the Frejus tunnel they had a smaller one but with an even lower background - he was invited to work there if he wished. Prof Jones replied that he was already discussing with Italian groups to work in the Gran Sasso tunnel (and also Los Alamos).

For the final question, I said there were hundreds of repeat experiments being performed, a few had positive results confirming the two Utah results and they called press conferences (some later retracting) but many had negative results and none of them had published - there were two reasons for this (1) it is very difficult to be sure you have not made any mistake and the first negative experiment will be closely questioned by colleagues and the media, (2) how can they be sure that there is not some secret trick that the two Utah groups used. Prof Jones assured us that there were no secrets, except perhaps that he used these special fused purified titanium crystals with sharp points. Thus if one repeats the experiment exactly, one should get the same result.

Dietrich Schinzel of CERN asked Prof. Jones a number of questions at the private meeting afterwards particularly on gain control and stability of detectors. Detailed answers were not available, but it was repeated that the results were erratic, sometimes falling off after eight hours and another time the signal and background all declined by 50%. The electrodes had not been specially heat-treated before the experiments. Also it was replied that LiOD had not been tried by itself; that the cocktail of salts used had been developed over months [again, what criteria were used if this was before the good neutron detector was available last December?], but were considered not to be optimised. The current density (probably the maximum) was 100 mA per cm2. The purity of the Ti was 99.8%

An interesting new piece of information was that in Run 6 that was described as having a 3 gram pellet of Titanium (page 4) or as having several fused pellets with a total mass of 3 grams (page 5), there apparently was also a palladium cell - and Prof. Jones discussed whether palladium could have been deposited on the titanium rod [looking at the drawing of the set-up, it would be interesting to know how it was determined that the neutrons came from the titanium and not from the palladium].

Unfortunately time was very limited and questions were many. Among the things that were missing from the paper were;

The paper says that "typically 4 - 8 cells were used simultaneously" and I wondered that their only really successful run, No 6, had only the single titanium pellet - now we learn that there was a second cell at least in place at the same time. It would be good to see a complete list of what electrodes of what size and composition, were in use for each of the 10 runs plus also a full description of the 4 day run not in the paper.

The current density should be given for each run and electrode.

OTHER COMMENTS

1. Although the price of palladium is rising fast, and annual palladium production is only about 100 tons per year, it should be noted that other materials also absorb hydrogen, e.g. titanium, some alloys...

2. Peter Igo-Kemenes who speaks Hungarian, phoned Prof Csikai of the Kossuth University in Debrecen, who had reported confirmation of the Utah results. He said they had only looked at neutrons and had obtained a (signal + background) of 0.12 neutrons/s while the background alone was 0.04 neutrons/sec. They will publish in one week.

NEW IDEA AND RESULT FROM FRASCATI Tuesday 18 April 1989.

This morning's Italian newspapers are full of a major discovery by a group led by Prof. F. Scaramuzzi of Frascati. The papers were rather short in details but Ugo Amaldi gave me what looks like a press release in Italian but which also contains two figures, and more usefully he described the experiment to me.

Basically they are trying a "dynamic" operation instead of the "static" work done up to now. That is they are trying to move the deuterons through the titanium in the hope that they will come closer together more often and so fuse. They take a cylinder, 3cm in diameter and 18 cm long and fill it with fine shavings of titanium. This is in deuterium gas. All is cooled to liquid nitrogen temperature as the amount of deuterium absorbed should increase at lower temperatures. A pressure of 8 atmospheres is applied. After a time neutrons are counted and this rate rises and then falls. In figure 2 of the paper, this rise starts after 200 minutes, reaches a peak of 300 counts/(per unit of 10 minutes - I think, but it could be per sec) and then falls to a background count of close to zero at time 1100 min. Later the cycle is reversed and the pressure is released and the titanium is warmed up to room temperature when again the neutron count rises and then falls. It is not clear whether fig. 2 applies to the first or to the second part of the cycle.

However in fig.1 there is a counting rate in hours and here again John Ellis and I have been trying to figure out what it means. It extends over 64 hours and shows a background counting signal of about 1 per 10 min. with bursts of signals of about 15 to 45 counts per 10 min. It is possibly impressive, but needs explanation (I will be at a WA84 Collaboration Meeting all tomorrow, so please ask others for faxes).

It is said that a peak counting rate of a thousand per second is reached and that the cycle has been repeated "many times".

Patent rights have been applied for. It should be noted however that the energy production is about a billionth of a watt, much less than the energy required to cool and heat and pressurise the titanium, i.e. it is very unlikely to be a source of energy production.

Eric Heijne, the CERN expert on Silicon, has given me some interesting graphs.

On the graph of absorption of hydrogen at one atmosphere as a function of temperature, titanium looks fairly flat at about 50 000 cm3 (NTP) per 100 gram, i.e. one would gain little by cooling. On the other hand the curve for palladium rises very steeply from 300 at 400 C to 800 at 200 C and 8 000 at 120 C, but then the curve suddenly changes and becomes both dotted and flat giving 9 000 at 0 C.

Looking at a graph of the diffusionn coefficients of H and D in the metals Palladium, Niobium and Vanadium (G. Alefeld, Comments on Solid State Physics, 6 (1975) 53), the diffusion rate always decreases with lowering the temperature. For D in Palladium, it is 3 E-8 cm2 per sec at -50 C and 8 E-7 at +50 C.

Hence it would be interesting to know whether the cooling has a positive or a negative effect on the production of neutrons.

Overall this seems an exciting physics result, though of probably no economic importance, and one looks forward to many more results on this "dynamic" approach.

Douglas R. O. Morrison.

26.

26.

Dear E632 and WA84 Colleagues, 19 April 1989.

COLD FUSION NEWS No 9.

There are a number of exciting new results that when taken together may help to clear some, but not all of the con-fusion.

We would like to emphasis again that one should treat the work as being of TWO experiments. Firstly fusion with the possible observation of its products such as neutrons, gammas, tritium and helium. Secondly calorimetry with the possible observation of excess heat. Later one will discuss the extent of any relationship between the two.

1. FUSION

Recent and less recent results will be given and an attempt made to understand many(but not all) of them with a hypothesis. This hypothesis will then be compared with the results presented by Fleischmann and Pons and by Jones et al.

1.1 Firstly praise to the Frascati group of Prof. Francesco Scaramuzzi and his collaborators of the National Agency for Alternative Energy in Frascati for not copying the two Utah experiments but varying conditions.

Their idea was that it was a "dynamic" effect and not a stable condition where you charged up the Palladium until the deuterium reacts. Hence instead of taking rods of several mm diameter, they used very small pieces of metal so that the surface to volume ratio was greatly increased. Further instead of using palladium as did Fleischmann and Pons, they used titanium as Jones et al. - and looking at the curves for the absorption of hydrogen, it can be seen that titanium absorbs much more hydrogen than palladium (certainly above 400 Kelvin, but as in Note No 8, it is not clear at low temperatures). They were rewarded by seeing 200 minutes after the entry of the deuterons was started, a very strong emission of neutrons but which fell to normal after about 1000 minutes. When the deuterons were released from the titanium, another burst of neutrons were observed. This is the first completely convincing neutron signal that has been seen here.

In Astrophysics, the first to see a new Supernova, star, galaxy, etc, and who sends off his telegram first to the International Astronomical Union is called the discoverer. Someone who found this earlier but did not report it is called the Pre-discoverer.

1.2 Secondly praise to the Hungarian group of Prof. Gyula Csikai of Kossuth University for their powers of observation. We spoke for some time today and he explained that they had found that they observed neutrons only in the first 20 minutes after switching on their electrolysis. Thus their essential experiment was to switch on for half an hour, stay off for an hour and then repeat the cycle. This was done about 20 times. they found that the

signal + background = 0.12 neutrons per second
background = 0.038 neutrons per second

He estimated the signal as being about three standard deviations. We look forward with interest to the results when this experiment is run for a longer time to obtain higher statistics.

1.3 Thirdly praise to the original discoverers of fusion of hydrogen in palladium, Dr. Fritz Panath and Dr. Kurt Peters of the Chemical Institut of Berlin University. They performed their experiment at the timme when it was suggested that the energy of the sun came from the burning of hydrogen to helium by means of Einstein's equation, E = Mc2. Thus they did their experiment in 1926! Their original paper is in Berichte der Deutschen Chemischen Gesellschaft. They say that they passed hydrogen into palladium and by spectroscopic means observed helium production at the rate of about 1 E-8 to 1 E-9 cm3 per day. They used palladium because it is a good catalyst of chemical reactions. they used Pd in asbestos, Pd sponge and Pd metal where they treated the metal to get a bigger surface. They concluded that the amount of helium produced depends on how the surface was treated. They tried to look for gamma radiation but did not detect any (it should be recalled that gamma detectors were not too sensitive in 1926). A result that palladium could transmute hydrogen into helium, must have sounded too much like the Alchemist's Stone, for they wrote a second paper in 1927 (same journal) where they describe the many attacks and the additional experiments that they did to answer these criticisms - finally concluding that helium had been obtained from hydrogen. They seem not to have continued this work (because of the attacks?) which is not surprising because deuterium and the neutron were not discovered until 1932. In addition to Palladium, they tried Titanium and other metals, but found Palladium best. (I am indebted to Jaques Trembley, Eric Heinje, and Horst Wenninger for helping to translate the papers).

Per-Olaf Hulth tells me that in Sweden, a famous scientist, Dr. Tanberg is well-known to have observed production of helium from hydrogen in 1927 and in fact his apparatus is still on display.

Have recently heard that Prof. Pons of Utah reported that they had found production of Helium from their electrolytic cell - do not know at what rate or if it was 3He or 4He.

1.4 METHOD OF PUTTING HELIUM INTO METAL

Recently Fleischmann and Pons and also Jones et al. have introduced a new way of putting hydrogen into metals by electrolysis and this method seems to have been successful in causing fusion. The Frascati group have returned to what may be called "The old-fashioned way" of using gas pressure and that has been in use for many decades and it is clear from their very significant results that this gives fusion.

Which is the best? Using pressure, ratios of 0.6 to 0.8 atoms of hydrogen per atom of palladium have been found. Prof Fleischmann said he has obtained 0.6 but when I asked him how he knew, it seems it was theoretical. Have tried recommending groups to try and measure this ratio. It is perhaps important to note that Frascati had to wait 200 minutes then they got neutrons for 800 minutes using the pressure method while the Hungarian group only got neutrons for 20 minutes and this would seem to favour the pressure method but the efficiency of the two neutron detector devices is not known yet. The Hungarian group also tried the pressure method but only with a low pressure of 40 to 50 mm of mercury and did not observe a significant signal. Since the effect depends on surface area, there is also the fact that Frascati chose a very large surface while Csikai et al. used a rod of 5 mm diameter and a tube of 5 mm diameter and 1 mm thickness (they thought the tube was better but this must be at the limit of their statistics). Also one used palladium at room temperature while the other used titanium at varible temperatures. Hence we really do not know yet.

1.5 HYPOTHESIS TO EXPLAIN FUSION RESULTS

The Univ. of Utah group emphasised that it was a volume effect and that the surface of the material did not matter. However they seem to have been referring to the possible heat excess and this was reasonable in their theory, however it may have diverted people doing the other experiment of looking for signs of fusion. Hence the credit of the Frascati group and of the Hungarian group in thinking that it could be a surface effect.

It would seem that the effect does not occur appreciably in the steady state but more when there is a change in the direction of the force causing the deuterium nuclei to diffuse through the metal.

Now there was a theoretical argument that worried me about fusion in palladium. The crystal is a face-centred cube and the distance between the nuclei of palladium is about 2.5 to 2.8 Angstroms and this is very large compared to the distance of 0.74 A between the nuclei in a free deuterium molecule. And with a separation of 0.74 A the fusion rate is calculated to be a very low value of 1 E-64 fusions per deuterium pair per second. Now it is not too clear just what is the distance between hydrogen nuclei in a palladium crystal, but they seem to take sites which are more than one angstrom apart (would appreciate more information on this) and hence their fusion rate would be extremely low. However when there is a movement through the crystal lattice, the deuterium nucleus must jump from one site to another and there is a chance that in the course of this jump, the separation distance between two deuterium nuclei becomes so small that fusion becomes probable. This would tend to happen when the pressure is applied or removed. The rate of fusion would depend on the number of sites occupied in that region and on the rapidity of the change. The driving force could be physical pressure or electrolytic-induced pressure. Which is best requires more experiments.

Thus a hypothesis is proposed in which fusion occurs when deuterium nuclei jump from one site to another and pass close to another deuterium nucleus already in a nearby site. Since there is always some diffuse movement this probably occurs all the time but will be at such a low level that it is not easily detected except when the driving force is changing rapidly when copious production of neutrons could be observed and indeed was by the Frascati group.

1.6 COMPARISON OF THE HYPOTHESIS WITH THE DATA OF THE TWO ORIGINAL EXPERIMENTS.

Prof. Fleischmann and Prof. Pons and Prof. Jones and his colleagues are to be praised for re-opening the possibility of causing fusion in a catalyst. They have created great interest in this subject and have activated many experiments thus opening up a new field.

In the experiment of Jones et al., it was stated that they were puzzled by the fact that their neutron yields were very erratic and often stopped after about 8 hours. There was a tendancy to ascribe this to their poor statistics and analysis method, but it could be that they were observing the transient nature of the phenomen observed by the Frascati group who had a larger surface area of their titanium. When Prof. Jones was here on Monday he gave us samples of his favoured form of titanium - it had many sharp points and hence a larger surface to volume ratio than the smooth rods that many groups used, though not as large a ratio as Frascati.

The neutron counting rate of Fleischmann and Pons is much higher than that observed elsewhere and this is hard to understand as fusion. There overall efficiency of 2.4 E-6 was exceptionally small so that their actual rate of counting was small and close to natural backgrounmd rates. Natural background rates can vary from place to place, e.g. people sometime put in shielding thinking to decrease the background, but this can increase it if muons are slowed down more and these then interact in e.g. water to give neutrons via the reaction of the decay negative electrons with protons to give neutrons and a gamma. It was very worrying that the background rate was established by taking a reading at a distance instead of the normal and safer way of taking it in exactly the same situation but with the assumed neutron producer switched off. Measurement of neutron fluxes is a very difficult job and is best left to experienced people or at least experts should be fully consulted.

The gamma ray distribution shown in their paper is most impressive and convinced many. But again people wondered why the background level was determined by taking measurements in a different position (5 and 10 meters were mentioned while 50 metres were stated for neutrons), instead at the same position and switching off the possible source. The gamma spectrum shown has a sharp peak centred at 2.2 MeV. A possible explanation may be provided by a story Dietrich Schinzel told me today. This morning they moved their electrolytic cell with heavy water and palladium electrode down to a basement where they had a gamma detector. The gamma detector immediately gave a very fine peak ressembling that of the Utah group but at the slightly higher energy of 2.6 MeV. The Health Physics group said that was a well-known effect seen when a gamma detector is placed near concrete as concrete contains thorium which gives off a 2.6 MeV line.

2. CALORIMETRY.

It is generally agreed that calorimetry is a difficult subject. The problems are to be sure that one has included all the factors in the problem and that the energy balance is counted from the start. For example one could try and establish stable electrolytic conditions and then make the heat balance. But there is the danger that one has neglected the fact that by pushing deuterium into palladium, the structure of the lattice is deformed and considerable energy has been stored up, so that really one should start from time zero, but this means that the effect is small compared to the large amounts of energy that has entered the system. Also as the lattice fills up with deuterium, the sites that are being filled change with time and changes of sites can lead to different energy states. Thus to really understand the matter it would be better to consult experts on Palladium hydrides.

3 COMPARISON OF RESULTS FROM FUSION STUDIES AND FROM CALORIMETRY.

The University of Utah group claimed to have observed 10 watts per cm3 being produced. This is an impressive amount and it was stated that the authors knew of no known chemical process that could account for this rate. Hence they concluded that it must be nuclear. But if it is nuclear then one would expect to observe the particles resulting from such processes such as neutrons, gammas, tritium. However their measurements gave rates which were many orders of magnittude less (more than a billion times less) and other experiments all give enormous discrepancies. If you do not observe the products then it is unreasonable to expect that a fusion process has taken place. However the authors concluded that "the bulk of the energy release is due to a hitherto unknown nuclear process or processes". This was a major sticking point to physicists that a nuclear process could give a billion times more energy than normal fusion. However helium had not been measured and it was speculated that the reaction could be

$$D + D = {}^4He + \text{recoil energy.}$$

However there are two arguments to close this unlikely loophole. Firstly the recoil energy must be taken up by something and the only thing available is the lattice of the palladium crystal. In the News note of 4 April, the crucial point was made that the timing is impossible - the nuclear reaction takes place in a such a short time that the lattice does not have time to move in a coherent way. Secondly Paneth and Peters already faced this problem in 1926 when they observed that the amount of Helium observed was many orders of magnitude smaller than the various terms in the heat balance equation i.e. there was not enough helium produced. Hence all possible nuclear reaction products have been covered. The most reasonable conclusion is that there is some problem in the very difficult calorimetry.

4. FUTURE EXPERIMENTS

There are many more interesting experiments needed, but they must be of high quality and in general should involve experts from different fields or at least experts should be continually consulted. Since it is clear that there is unlikely to be any economic interest, it is to be hoped that secrecy and first publication via press conferences can from now on be avoided.

4.1 There are many obvious experiments involving the two methods of obtaining the force to drive the deuterium into the metals using pressure and electrolysis and comparing them. I have already heard of some ingenious ways of doing it and people are sure to have other new ways of doing it, the subject is so fresh. Some special suggestions are;

4.2 Approximately equal mixtures of tritium and deuterium should be tried as the barrier penetration probability (i.e. the cross section) is so much greater than for the deuterium - deuterium system.

4.3 Factors affecting the fusion rate would be expected to include

4.3.1 the position of the hydrogen sites in the metal and the manner in which the hydrogen nucleus jumps or flows from one site to another - this needs consultation with experts in metallic hydrides.

4.3.2 the amount of hydrogen that can be absorbed by the metals, here Titanium, Zirconium, Cerium, Vanadium, Palladium (but only at temperatures below about 200 C), Tantalum, and Niobium are possibilities.

4.3.3 the rate of diffusion of hydrogen and deuterium nuclei through metals (they are different being much faster for hydrogen in Vanadium and niobium but in Palladium deuterium is, surprisingly, slightly faster). There are two points; the diffusion rate is very temperature dependent, e.g for deuterium in Palladium it is 2 E-8 at -60 C and 8 E-6 cm2/s at +200 C so the higher the temperature the higher the diffusion rate but palladium is particularly bad for the absorption of hydrogen falling steeply with rising temperature. Vanadium has a very high diffusion coefficient,D, 1 E-5 at -100 C and 1 E-4 at +200 C (the relationship is linear on a plot of D against 1/T where T is in K). Hence for their higher values of D, Vanadium and Niobium would perhaps be worth trying.

5 CONCLUSIONS

It is clear that there are two very different experimental results and initially there was great confusion by mixing them. The "result" that there was an unknown source of heat giving 10 watts per cm3 of palladium should be considered mistaken. There is a second class of experiments which indicates that in certain circumstances fusion can take place inside a metal catalyst but the rate of heat production is so extremely small, about 1 E-10 watts, that even with extensive developments, it is inconceivable that it could produce anywhere near as much heat as is required to make the fusion take place. Thus with great regret, it is clear that cold fusion can never be a practical source of power as you have to put in much much more power than one gets out. So our best hope for long term power production with the smallest amount of pollution' is still "hot fusion" with temperatures of 100 million degrees, but this serious development work is expected to take several decades.

6. OTHER POINTS

6.1 SOCIAL RESPONSIBILITY

Everyone dreams of an inexaustible source of energy which creates very little pollution. Anyone presenting experimental results showing this might be feasible, gets all encouragement and lots of attention. "Cold Fusion" started this way. One was aware of the social responsibilty of scientists but it was brought home to me very strongly by the cartoon in the International Herald Tribune of 4 April which shows the oil spill in Valdez in Alaska and a buoy floating on which are two seals and a bird covered in oil and the bird asks "Any more word on how those fusion experiments are going?"

I heard that in a nearby country there were two groups starting an experiment to repeat the original ones and not finding anything. Then on the television news they suddenly heard that in a small town a group had repeated the experiment and found neutrons! They quickly enquired and asked for details and learnt that a Geiger-Muller counter had been used (it is famous but does not count neutrons). They quickly told the media and others and the false news did not escape from the country. Now in how many other countries does there exist the possibility for responsible scientists to respond and act quickly?

6.2 There are strong rumours from several directions that the Utah group have done the control experiment so often requested, to use ordinary water instead of heavy (deuterated) water and use their method of calculating It is said that they found a heat excess! It would be good if this news were confirmed as if it is-true it would clearly establish that the original work on calorimetry was mistaken and there is absolutely no connection between any heat measurement and fusion.

6.3 Several people have asked me how I got the Paneth and Peters paper of 1926 The story is typical. A friend at the Max Planck Institute in Munich, Mike Aderholz, is a member of the E632 experiment at Fermilab near Chicago, and he gave one of these news notes to a friend in the Plasma Physics section. This friend took it to a conference in Oak Ridge where they were discussing it. A Russian who was there said "Why do you not read your own literature?" and gave the 1926 reference. This came back to Munich and Mike sent me a Telefax of the two articles yesterday afternoon. It now seems to be circulating very quickly and some people have asked me if I have seen it.

6.4 The rise in the price of Palladium seems to have stopped. It is always possible it will rise again as its unusual propreties have been brought to wide attention and maybe some one will find other uses for it.

6.5 I still keep thinking about the two maps of the world that Prof. Jones showed on Monday of the heat flux coming up through the earth's surface and the relatively high rates of the 3He to 4He gases emitted. These showed the highest heat flows in the Pacific Ocean well off the coast of Peru - it would be interesting to know how this fluctuates with time and if there is a relation to the temperature of the nearby ocean water as this temperature seems to drive much of the world weather.

Douglas R. O. Morrison.

Dear E632 and WA84 Collaborators, 24 April 1989.

COLD FUSION NEWS No. 10.

1. Announcement of International Conference on Cold Fusion
2. Frascati and Rome Results on Fusion - "Dynamic or Non-equilibrium Effects"
3. Other New Results and Retraction on Fusion
4. New Results and Retraction on Calorimetry
5. History of Early Ideas and Work on Fusion before 1940
6. Talk by Prof. Rafelski at CERN and Discussion
7. Comments on the two Original Papers
8. Structure of Metal Hydrides, d-d Separation
9. Conclusions

1. INTERNATIONAL CONFERENCE ON COLD FUSION.

Prof Rafelski announced that a conference on cold fusion would be held at Santa Fe on 23, 24, and 25 May and it was intended to be an International Conference. Have no further details.

2. FRASCATI AND ROME RESULTS ON FUSION - "DYNAMIC or NON-EQUILIBRIUM EFFECTS"

A first description of results obtained by the Frascati group of Prof. Scaramuzzi at The National Institute for Alternative Energy was given previously based on a press release with two graphs attached, one of which seemed to show a very significant and believable effect and the other one which looked unusual and was not interpretable.

Ugo Amaldi was able to provide more details from a talk Prof. Scaramuzzi gave in Rome. The graph dated 7 to 10 April is rather complicated. It covers a period of 64 hours. At 4 hours a pressure of 50 atmospheres (not 8) was applied to drive deuterium gas into the tube containing the titanium shavings whose weight was 100 grams. From 12 to 18 hours neutrons were counted then the rate fell to almost zero. At 19 and 21 hours there were fresh bursts of 40 to 50 atm. of deuterium gas which were followed by a sustained flux of neutrons until 31 hours, some between 33 and 36 hours and between 39 and 42 hours when the pressure was removed and the neutron counts continued until 50 hours but with a two hour gap from hour 44 to 46.

At first sight it looks as if something is really happening, but when one follows the chronicle of the changes and counts, it is hard to interpret rationally - though this does not mean there is not some other factor that we are unaware of.

What is much more worrying is the distribution of the counts which seems quantised with groups near 20, 40 and, I am told, one count at about 60. There are no counts between 3 and 16 and none between 25 and 36. Further if one divides the time into three intervals, 11 to 18, 22 to 31 and 33 to 50 hours then the centre of each group near 20 and near 40 increases with time. The graph is not to easy to read, but I find roughly;

Time, hours	Group near 20 counts	Group near 40 counts
11 - 18	Average = 19	38 (one entry)
22 - 31	20	Average = 40
33 - 50	22 1/2	43

In each of the three intervals there were quite a number of intervals with 0, 1, or 2 counts as was the "background".

Thus overall it looked like the superposition of two Poisson distributions, one with 0, 1, 2 and a 3 and the other "integers" with 0, 20, 40, and a 60.

The other graph looks very impressive. Here the container with titanium was charged for some long time with D2 gas at 10 to 20 atmospheres pressure, then at time zero put into a bath of liquid nitrogen and the D2 gas was pumped out. After 220 minutes the neutron counting rate increased from a background of about zero counts to a peak of about 320 counts over 10 minutes, then the rate steadily fell to about zero at 1100 minutes. This time there was no sign of any quantised effects or of unexpected drops to zero counts.

The counter used was a boron fluoride gas counter after paraffin - this is by modern standards rather a simple one as it does not require coincidences (with a liquid scintillator counter with lithium inside it, there is a first pulse as the neutron makes collisions knocking protons forwards as it slows down, then there is a second pulse as the neutron is captured by the Lithium to give tritium and helium. Such coincidences are excellent signals of neutrons and greatly reduce any background or instrumental effect).

David Williams of Harwell suggested that if the BF3 counter was not physically shielded from the cell, the coldness of that system could cause condensation on the counter - these are known to be very temperature and moisture dependent - and could give impressive results rather like the effects observed. It would be good to answer this possibility by carefully controlling or varying the conditions (e.g. by varying the distance to the BF3 counter) or by using a scintillating liquid/Lithium counter. One hopes that the possibility that David raises does not apply, but it must be checked.

Ugo said that the efficiency of the neutron counter (geometrical plus the actual counter efficiency) was 5 E-5. This is a rather low number compared with the tens of percent of some other counters. This means the actual counting rates were often much less than one per second and close to the rates of Fleischmann and Pons and of Jones et al.

These comments must be considered tentative and await further work by the ENEA group at Frascati. Have tried to telephone but have had no return call. However there is one important point that can be made. A major comment about the Fleichmann and Pons work was that controls had not been made before the Press Conference. Here no controls have been reported, i.e. the experiments have not been repeated with inert electrodes, e.g. iron or glass, and the experiments have not been repeated with hydrogen or helium in place of the deuterium gas.

The basic idea behind these experiments is that fusion does not occur with measurable frequency in stable conditions but it might occur at higher and measureable rates when there is a sharp change of conditions, that is, in non-equilibrium conditions.

At the same meeting, an experiment was reported performed by Prof. Perfetti et al. of the Istituto di Struttura della Materia, CNR, which is near Frascati. They used Pd or Ti cathodes and Pt anodes with electrolytes of D2O + LiOH or D2O + LiOD. The typical charging current was 8 mA/cm2 and they charged for 12 hours. Neutrons were measured (have no details of counters except that they were of low eficiency). The cathode was small, 0.1 g of Pd. By weighing it was found that there was 0.6 atoms of deuterium per atom of Palladium, as Prof. Fleischmann had claimed (but not measured). A current of 10 Amps was passed to raise the temperature suddenly to 150 C. After three minutes a burst of neutrons was observed for 10 minutes. This was repeated three times. About 25 counts were recorded each time. There seemed to be zero background. With titanium the results were different - the background was 2 counts per min. (why the difference?) and the "peak" afterwards did not look very significant. It seems this was only performed once for 40 minutes. There were no reports of any controls or of longer runs being performed to give statistically significant statistics.

In conclusion, refereed papers with reports of controls, are needed.

3. OTHER NEW RESULTS AND RETRACTION ON FUSION

There are a number of reports of results confirming the original reports of Fleischmann and Pons and Jones et al. Thus my Brazilian neighbour told me that at Sao Paulo the group of Prof. Nasciomento had found an excess of neutrons when using D2O and Pd.

Most of these results seem to come from press releases. They are summarised by Tom Wilkie, the Science correspondent of the Independent of London in another sensible article. He quotes reports of confirmation from South Korea, East Germany, Czechoslovakia, Moscow, Brazil and a couple of students in Seattle.

On the other hand a report of confirmation from US labs has been withdrawn. Georgia Tech found their neutron counter had variations due to temperature fluctuations.

At Harwell David Williams and his group have been doing a large series of experiments with controls. He has said that they do not observe any excess of neutrons or of gammas.

During his visit to CERN, Prof Rafelski left a note saying that Sandia finds of the order of 1 E-11 atoms of both 3He and 4He per gram of palladium, corresponding to a fusion rate of E-17 per sec per atom, but only in the heavy water electrolysis. Livermore sees of the order of E-10 atoms per gram of Pd of 4He only - nothing was observed for light water. See Chapter 5 for earlier work on helium production.

A preprint by two Utah chemists who propose a model for cold clean fusion has been sent to me. It says "Pons and Hawkins have informed us that, in a preliminary experiment, mass spectroscopic analysis of evolved gases from a cell operating at 200 mA with an electrode volume of 0.0785 cm3 and delivering 0.5 watts/cm3 of excess heat, showed a 4He/d2 ratio of 10E-5 to 10E-6 and substantially larger than that of a number of blank determinations. This corresponds to 8E11 to 8E12 atoms of 4He/cm3 sec, certainly of the same order of magnitude as (actually larger than) the value of 1.3E11 4He/cm3sec obtained by scaling the number 2.6 E12 calculated above by the excess heat ratio 0.5/10."

4. NEW RESULTS AND RETRACTION ON CALORIMETRY.

Robert A. Huggins, a professor of Materials Science and Engineering at Stanford University, reported that his team of 6 people had used up to 5 electrolytic cells filled with both D2O and H2O. They found excess heat with the D2O but not with H2O. Also the amount was "in the general range" of that reported by Fleischmann and Pons.

On the other hand, there are persistant reports at a very high level, that at the Univ. of Utah, Prof. Pons has repeated the calorimetry measurements with water and finds comparable excess heat to that they obtained with heavy water (though with perhaps slightly less in water than in deuterium oxide).

David Williams of Harwell has said that in repeating the calorimetry work, they have found no effect. He expects to publish fully soon.

The Texas A and M university group that had reported observing about 40 to 80% excess heat, had trouble repeating their work. They have now withdrawn their results and say that there was an electrical fault.

5. HISTORY OF EARLY IDEAS AND WORK ON FUSION BEFORE 1940.

The idea of fusion of hydrogen in palladium metal has a long history. In 1926, Paneth and Peters of Berlin first suggested it and obtained positive results (Berichte der Deutschen Chemischen Gesellschaft vo. 59 (1926) p 2039). They say that most of the published claims of helium formation by electrical discharges have already been disprovved by more accurate measurements, but they felt their method was more accurate. Quantities between 10 E-6 and 10 E-9 cm3 of helium were found. After many criticisms, they withdrew many though not all, of their results because of contamination of asbestos and of glass (B. der D. Ch. G. vo 60 (1927) p 808). Then in the May 14 edition of Nature, they withdrew all their results. This they then followed with two long papers (ZS. f. phys. Chem. (B) 1, (1928) 170 and (1928) 353 describing their apparatus and all the tests they have done pushing their limits to 1 E-10 cm3 of helium (this is the correct way to withdraw results that have caused great public interest). The main problem was that glass can absorb minute amounts of helium quickly and allow the helium to diffuse through it. They were very careful experimenters and their papers are clear, describe many controls, and are a pleasure to read. Thanks to Mike Aderholz for sending me the papers from MPI Munich.

Per-Olaf Hulth has given me more information of interest about a famous Swedish scientist, John Tanberg. He suggested in 1927 that it would be better to use electrolysis rather than gas pressure to drive hydrogen into palladium. In a biographical book published in 1970, it is described how he applied for a patent and was refused, but in 1932 after deuterium was discovered, he obtained heavy water from Neils Bohr. First he filled the palladium rods with deuterium by electrolysis, then he applied a large electrical current very suddenly to heat the palladium, hoping this would cause fusion. If all the deuterium had fused to give helium, he calculated that the explosion would be equivalent to 1000 kg of dynamite and hence told his workers to go home the nights of his tests. However he found no effect. His apparatus is said to be still on display at the Royal Technical Institute in Stockholm. While he clearly showed that a substantial fraction of his deuterium did not fuse, modern detectors are more sensitive, so that it is not excluded by his experiments that some small portion could have in fact, fused.

6. TALK BY PROF. RAFELSKI AT CERN AND DISCUSSION

Prof. Rafelski of Arizona is an author of the paper by Jones et al. and simultaneously he, Prof. Jones and others, published a theoretical paper on limits on cold fusion of d-d isotopes.

He told us that their paper on observation of cold fusion had been accepted by Nature. The Wall Street Journal had just said that the Fleischmann and Pons paper had been withdrawn (Nature writes that requests for changes were made to both sets of authors). He described the history and said they had obtained an effect of 5.5 standard deviations (4.2 if a limited number of runs are taken with background).

He reported confirmation by the Hungarian group of Csikai et al. and by the Moscow State University group of Kuzmin et al. who had 20 successful runs with a neutron flux 5 times background, and he also reported the Frascati work.

He described their theoretical work and drew a picture of the palladium lattice with the deuterium nuclei in a close ring in a manner different from the normal model of palladium hydrides described below (this does not necessarily make his model wrong). He emphasised that we should not ignore the calorimetric results of Fleischmann and Pons just because it was about nine or more orders of magnitude from the measurements of neutrons and in particular noted that the reaction p + d ---> 3He + gamma, is 1 E6 times more probable than the normal fusion reactions such as d + d ---> 3He + n, and D2O is never pure but contains also H2O (however in Fleischmann and Pons's experiment they have 1/2 % of H2O and hence the gain of 1 E6 has to be reduced by 0.5 E2 and is still far from the desired factor of nine or more orders of magnitude).

John Ellis had asked me to give a five minute summary of other work. Afterwards there was a discussion which might be called "full and frank". At the end I tried to summarise by saying that if there were excess heat, chemists consider it not to be of chemical origin and particle physicists consider it not to be of nuclear physics origin. This position has already been described by a famous 18th century Scottish philosopher, David Hume, who wrote (approximate recall), "Would you rather that all the laws of Nature be violated or believe that one man made a mistake?".

7. COMMENTS ON THE TWO ORIGINAL PAPERS

The paper of Fleischmann and Pons had a very impressive and clean peak of gammas at 2.2 MeV. After the previous comment that Thorium in concrete gave a gamma of 2.6 MeV, both David Bailey of Toronto and Steve Errede of UI Urbana, discuss gammas from decay chains of radon, thorium etc. Art Rosenfeld, (who started the Rosenfeld Tables - now PDG Tables) switched in 1973 to Energy at Berkeley and has greatly emphasised to me the dangers of radon being pulled into the basement from the ground below by the heating system creating an under-pressure. The result is that the concentrations of radon in the basement of a building can vary in unexpected ways. David Bailey writes that in the radon decay chain, Bi(214) decays to Po(214) which emits a gamma with 5% probability, and the gamma energy is 2.204 MeV. It requires a good spectrometer to distinguish this from the 2.224 MeV gamma from neutrons stopping in water, n + p ---> d + gamma. Both writers note that the gammas observed by Fleischmann and Pons actually appear to peak at 2.204 and not 2.224 Mev as expected by them.

During the discussion after the talk by Prof. Jones at CERN last week, there was considerable criticism of the way in which the statistical analysis was done, in particular the effect of the low statistical sample of the background and the fact that it had to be used to scale the background under the peak at 2.4 MeV. On looking at fig. 2 of the Jones et al. paper, I could not understand this as the fluctuations of the background were very small, and I normally take this as an indication of the size of the statistics and counting more closely saw there were hundreds of counts. However what I had not noticed was a sentence in the figure caption "Note that ten counts have been added to each three channel bin for clarity of presentation". Thus when the counts were shown as 10, 11 or 12, they were really 0, 1 or 2. Thus the comments on low statistics were justified. This is a most unusual way of plotting data - the conventional way is to increase the size of the bins in the region of low statistics until the numbers per bin reach 5 counts which is generally considered to be the beginning of statistics.

During his talk, Prof. Jones gave some numbers that were not in the preprint. These were rates for the foreground (signal + background) for the channels near 2.5 MeV and for the background taken as > 2.7 MeV;

	Rate in 2.5 MeV	Rate > 2.7 MeV
Foreground	0.00307 +/- 0.0001	0.00140 +/- 0.0001
Background	0.00245 +/- 0.0001	0.00140 +/- 0.0001
Subtraction x E-4 /s	6.2 +/- 1.3	Same

This was the basis of the claim of more than five standard deviations.

It is left as an excercise to the reader to recalculate the error on 6.2 E-4 when one takes into account the error on the determination of the scaling factor from the rates > 2.7 MeV.

8. STRUCTURE OF METAL HYDRIDES, d-d SEPARATION.

Have received a drawing with distances on it from Prof. Pui Lam of Hawaii and had a long conversation with Keith Ross of Birmingham. It is such a pleasure to discuss with a real expert.

Basically Palladium is a face centred cube with side 3.89 Angstroms. When hydrogen enters it goes into an octahedral site until they are full. This sounds very complicated but in fact this is the same structure as sodium chloride where the atoms are a series of cubes with Na and Cl atoms alternating - it is shown on page 8-43 of Condon and Shortley's handbook. At higher concentrations of deuterium, the tetrahedral sites are next filled. Using the indices (a, b, c), then palladium would be at (1/2, 1/2, 0), the octahedral sites at (1/2, 0, 0) and the tetrahedral sites at (1/4, 1/4, 1/4). Hope this is clear though it usually takes a bit of model-building with hands and paper and pen. The essential point is that in these steady states, the distance between the deuterium nuclei is large, 2.85 Angstroms for two d in octahedral sites and 1.74 A for octahedral-tetrahedral sites while free deuterium molecules have a smaller separation of 0.74 A - this means that the probability of fusion is infinitely small. It perhaps shows that when one starts on a new subject, one should consult experts or read the literature.

Many people, myself included, have hoped that when a deuterium nucleus jumps from one site to another, e.g. when diffusing through palladium, that there was some chance that two deuterium nuclei might reduce their separation sufficiently to have a reasonable probability to fuse. So far I do not know of any calculation, but the distance between the nuclei seems so great that it is difficult to be optimistic. Maybe one should consider the present set of experiments searching for fusion as an elegant way of setting limits.

Could there still be some better situation? Keith Ross suggested Lanthanum hydride, LaH3 where there are three hydrogen ions. Another possibility is that when there are other metal salts, e.g. iron, there is a certain amount of deposition of Fe on the surface of the (palladium) electrode and it might be that the ions are closer together then, but he (and myself) were rather doubtful.

Useful books on metal hydrides recommended include , G. Alfeld and J. Volkl "Hydride Metals", Springer, two volumes.

9. CONCLUSIONS

For the case of "stable" or steady state cold fusion as described by Fleischmann and Pons, we seem to be in Phase two of the normal history of a wrong scientific result, where there are some confirmations of the original findings still being reported but retractions and negative results are also appearing. If the scenario continues, Phase three where there is an avalanche of negative reports, will begin soon. The experimental results and theory of metal hydrides seems to be in agreement with the negative results.

A new concept is of "non-equilibrium" or "dynamic" emission of neutrons. There are few experiments and as explained, their significance will not be clear until reports of all the checks and blank runs they have made are available. There are probably no calculations on this subject.

In some ways it is tough writing these notes. Great hopes are raised, dreams seem to be realised mainly on the basis of press conferences, of inadequate information. There is confirmation from some other group, again the information comes from press conferences. But one is "fortunate" to have good access to more solid information, and one realises that these first experiments are poorly performed with few checks or controls, and probabaly give wrong results. The conviction grows that they are all wrong results. But people keep coming into my office and telling me that in their country people are repeating the original experiment and finding the same result. One wants to help them and ask them to please be cautious and check very carefully first before exposing themselves.

Douglas R. O. Morrison.

Some places in the States have trouble with my preferred mailing address of
MORRISON@VXPRIX.DECNET.CERN.CH
Another address that could be used is;
DROMCD@VXCERN.DECNET.CERN.CH

PS The Radio Suisse Romande has just telephoned to say that there has been a communique stating that a team from Grenoble has placed their electrolytic cell in the neutron detector at Bugey and has found no effect. This is the detector that I know well and which is of high quality with liquid scintillator in which there is Lithium. The preliminary results were already below the Jones et al. limit.

PPS Yves Declais of Bugey has given me some more details. Their limit for neutron production is more than a factor of a hundred less than the value of Jones et al. and many orders of magnitude less than Fleischmann and Pons. The paper will be issued shortly. They are moving the cell to the Frejus tunnel where the neutron counter is smaller but the background is exceptionally low as there is little natural radioactivity, of the order of one count per year!

39.

Dear E632 and WA84 Collaborators, 30 April 1989.

COLD FUSION NEWS No 11.

1. Summary of the Situation

2. APS Session on Cold Fusion

3. Results and Ideas on Calorimetry

4. Results and Ideas on Neutrons

5. Results and Ideas on Tritium

6. Theories

7. Regionalisation of Results;
Correlation between Results and Information Available

8. Historical Perspective

1. SUMMARY OF SITUATION

Out of the hundreds of experiments being done, more are beginning to report so that now the situation can be evaluated in two ways,

(1) by simply counting the numbers confirming or finding no effect

(2) by judging by the quality of the experiment - is it known that adequate checks and controls have been made? are the counters reasonably efficient? have acceptable statistical errors been given?

The first is easier to do. For example Le Figaro of 28 April had a front page story "Treize reussites, treize echecs" which was followed on the inside by a table listing 26 labs that had reported in a manner accessible to the newspaper. The table had four columns listing lab's name, confirmation?, energy(calorimetry) and neutrons. Although the headline suggested the score was 13 to 13, Georgia Tech was quoted as "oui puis non" and scored as "yes" whereas one expects they would prefer to be taken as "no" i.e. 12 to 14. Also Texas A and M are scored as "yes", but they have since retracted their confirmation, i.e. 11 to 15. There is another experiment that I recognise where the authors withdrew after learning that Geiger counters have a rather low efficiency for neutrons, i.e. 10 to 16. On the other hand Tom Wilkie of the Independent (the new serious paper of London) lists confirmations of minimal amounts of neutrons from South Korea, Brazil, and East Germany, and a couple of research students in Seattle i.e. 14 to 16. But a Berlin group can explain an apparent emission of heat i.e. 14 to 17. Thus we are passing through Phase 2.

The Economist quotes Prof. Pons as saying that "60 laboratories have privately confirmed different parts of his and Dr. Fleischmann's work", but as there are no names of labs given, nor details of the checks done, it is difficult to evaluate this statement but probably this will be possible at the APS meeting tomorrow.

Quality is always more controversial, but the few experiments that I know to have made reasonable checks are all "no". In addition as details emerge of the four original experiments of Utah University and the one of Brigham Young University, they are all being strongly criticised (see No. 10 and below).

A striking pattern is emerging in which in some regions of the world, the experiments all give "yes" and in others they all give "no". There may be some correlation between the information available in a region and the results obtained there.

There are worrying signs of disunity in the scientific community and it is important in the future that people behave responsibly and even more importantly with kindness.

2. APS SESSION AT BALTIMORE

On Thursday afternoon I received an electronic mail message that seems to have originated on Wednesday afternoon and was sent by electronic mail only. It said that "As a result of the excitement produced by the the recent cold fusion experiments, the American Physical Society has agreed to organise a special session on the topic at the Baltimore meeting of the APS". It will take place on Monday May 1 from 19.30 to 23.00, the room to be announced. It is scheduled that there will be four half-hour talks by S.E Jones, M. Fleischmann or B.S. Pons, J. Rafelski, and S. Koonin, and the remaining 90 minutes are for contributed papers. Abstracts for contributed papers should reach the APS in NY by noon on 28 April or at Baltimore up to noon on May first (have been informed that electronic mail and telefaxes are not acceptable). Foreign scientists are specially invited.
I tried to pass the news on to European scientists who have done or are doing some of the most interesting experiments, but at that moment all said it was not possible to attend, but indicated what I could say at Baltimore.

3. RESULTS AND IDEAS ON CALORIMETRY

At TIFR, India, a first test has been done using 20 ml of D2O and with NaCl as electrolyte. The temperature rose quickly and then stabilised. Their preliminary indications are of a heat excess but no error is mentioned to say whether this is significant or not. Also no clear analysis of Newton's law of cooling has been done. A nichrome wire heater was used as controlFull checks or control experiments have not been done yet. Some workers have reported trouble with such type of electrolyte due to effects produced by the Chlorine gas boiling off. (This has not been announced and thus should be taken as very preliminary, and hence is not counted above.)

For the Stanford experiment of Dr. Huggins, I am now told that they observed 50% more heat with D2O than with H2O and from which they deduce that there must be something non-chemical happening.

I have no details about a report that Tokyo University and the Ghandi Centre for Atomic Research at Kalpakham in India have both reported as observing excess heat (Le Figaro, 28/4/89).

George Yost told me of a meeting at Berkeley where an electrolytic chemist commented unfavourably on the Fleischmann and Pons calorimetry, in particular their use of an "open" arrangement where the gases can escape. He said that Pons had said that they ran for 120 hours at 5 Volts and this gives a measure of the energy put into the system which is not incompatable with the 4 megajoules talked about. The Stanford Experiment also suffers from being an "open container" one. He offers a possible model to explain the Stanford results - it is known that it is much easier to dissociate H2O than D2O (used to separate isotopes). Thus for the same amount of energy in, more of the light hydrogen bubbles off (and this is not easily recognised in an open experiment) so that the energy going into the heavy water, goes less into dissociation and more into heat production (I think that one has to ask Stanford people about the accuracy of their measurement of the water levels and how this was fed into their calculations).

David Williams of Harwell described to me their calorimetry system. It is well known that the most reliable measuring instrument is one that measures nothing! -for example the Wheatstone Bridge. So they have three cylinders at three temperatures. Heat is added to enable the temperatures to be stable - i.e. it is a null method and Newton's Law of cooling is not required. If there were to be excess heat, then the external heat added would have to be decreased to maintain constant temperature. They are studying both open and closed systems. They are also using various controls. They intend to do a very thorough job and find that as new techniques (e.g. Frascati) are reported, they have to extend their work. He hopes to be ready to publish all at the end of May. He told me another interesting thing - that Li, which is often used as an electrolyte, oftens contains some potassium 40 which is radioactive and gives gammas (I remember it is also a nuisance in glass).

The Frascati group of Prof. Scaramuzzi say in their paper that they did not make specific calorimetry measurements, but if an excess heat had been emmited of the amount claimed by Fleischmann and Pons, then the liquid nitrogen in the bath around the cell, would have been evaporated anomalously, and this they did not observe.

Have been told by several people, but have not seen the report in German myself, that in Berlin they tried calorimetry. They noticed that as the level of the D2O fell, more of the palladium electrode got exposed to the air and the deuterium gas began to be emitted from this part of the palladium. This released the strains in the palladium rod caused by the inserting of the deuterium nuclei and caused a heating. They suggest this may be the explanation of why heat may apparently be given off. This should be easy to check.

I was reminded that heat produced from changes in crystalline structure of Graphite in a graphite-cooled reactor was reponsible for the Windscale fire that caused so much trouble - this is called the Wigner energy. A similar effect could be responsible for the emmision of heat when changes occur in the distribution of deuterium nuclei in palladium. It has also been suggested that changes between octahedral and terahedral sites of deuterium, could propagate along fault lines in the crystal causing regions of the crystal to release energy suddenly. Note that these are explanations which may not be considered chemical by some chemists, and also are not from nuclear reactions.

4. RESULTS AND IDEAS ON NEUTRONS

A group of electrochemists from Grenoble have made a cell which has been placed in the high quality neutron counter of Bugey. No effect was seen. The paper is being prepared (roughly it should indicate an upper limit about two orders of magnitude less than that of Jones et al. This experiment is now being repeated in the Frejus tunnel where the background is very low (one count per year, it is said).

At Jussieu in Paris, no evidence of neutrons was found.

At the Erice meeting, M. M. Broer of AT&T Bell labs, reported that with a 0.1 cm diameter and 10 cm long rod which was electrlysed for 10 days, the upper limit on neutrons was three orders of magnitude less than those reported by Jones et al. Also J. E. Zeigler of IBM said that they had upper limits of 1 E-3 /s/cm3 for the detection of t or p from the d + d reactions. An experiment from CNR at Frascati reported that neutrons were observed for the first few minutes - as reported in News No 10, however having seen these results, they do not look significant.(reports from Nature, 20 April 1989).

I have now had a sympathetic talk with Prof. Scaramuzzi who is the leader of the team at Frascati that reported non-equilibrium emmission of neutrons. He confirmed that they were not able to do control studies before their press conference but they were now doing them. He has sent me a preprint of their work which gave details. In the 7 to 10 April run, the titanium was degassed and a pressure of 50 atm achieved in several steps and then the cell was lowered into a bath of liquid nitrogen which slowly evaporated to quite a low level. The arrows on the graph correspond to times when nitrogen was added. The upward arrow after 42 hours was when the pressure was released. the authors had also noticed the "quantised" structure of the counts in units of 20 (but do not comment on the drift to higher values) and they suggest an interpretation in terms of counter saturation if the neutrons only come in very short bursts - however this seems to me to raise more questions than it answers. They will check this. For the second run, 15/16 April, "the deuterium had been in contact with the titanium bed at different temperatures and pressures for roughly one day and counts only just above background had been detected". to study desorption, "the deuterium was evacuated from the system by vacuum pumping and the liquid nitrogen dewar was also removed, allowing the cell and its contents to rise to room temperature. This moment corresponds to time zero in fig 3." The spectacular and very significant gaussian shape rise and fall of the neutron counts was then observed betwen 3 and 18 hours. The suggestion of David Williams that this effect could be due to cold and moist air affecting the neutron counter will be checked. This is the first time that I had appreciated that there was a day of running beforehand during which time no significant neutron counting rates were observed - it would be good to understand this. The overall counting efficiency is given as 5 E-5, which is rather low compared with tens of % in some other experiments. They conclude that these experiments "open an interesting field of scientific investigation".

David Rogers of the Canadian Ionising Radiation Measurements Standards Laboratory says that their group is trying to reproduce these Frascati results and have used 8 atmospheres pressure so far. They have a null result.

5. RESULTS AND IDEAS ON TRITIUM

At the Ecole Polytechnique of Lausanne, Claude Friedli and Victor Lopez, have measured the emmision of tritium from heavy water and found an effect when electrolysis was done over 100 hours with a current of 0.5 Amp. per cm2. When the palladium electrode (0.1 cm diameter and 10 cm long) was replaced by a platinium electrode, emmision of tritium continued at the same rate. I hope to get more details from Christos Comninellis tomorrow on the way to the plane which will allow an opinion on the significance of this result. The authors feel that the tritium is produced by a simple effect - the different electrolysis rates of deuterium and tritium.

It is interesting to note that Georgia Tech. originally said at their press conference that they confirmed the Fleischmann and Pons result by observing both neutrons and tritium. However the retraction only seemed to apply to neutrons - should we assume that the tritium result has also been withdrawn?

6. THEORIES

There are a number of theories which assume that the results are false and these have been given above (e.g that the "heat excess" observed was a short term phenomenon caused by the energy stored in the crystal lattice of palladium when it has been charged with deuterium).

There are also a number of theories which assume that the results are true.

6.1 Two Utah chemists propose that helium is formed in an excited nuclear state which is then de-excited not through the normal modes but rather by "internal conversion", i.e. processes involving overlap of electronic wave functions with the nuclear wave function. they propose that the same proces that causes the fusion (electron mass enhancement due to solid state effects) causes also the increase in the internal conversion rate. However it is estimated that the internal conversion rate should be many orders of magnitude less.

6.2 Dr. Peter Hagelstein of MIT has written four papers and MIT has subsequently released a summary of his papers and filed a patent application (Nature). He is said to consider the lattice of palladium and the deuterium atoms as a whole and considers the energy flowing from the fusion of d + d to 4He to flow freely to the lattice. If a cosmic ray muon causes one fusion by muon catalysis, then the spread of the energy would help other fusions causing a chain reaction (hope I have got that right, but I am not sure). However such ideas seem to ignore the time factor - as written earlier, the nuclear reaction happens many orders of magnitude faster than the vibration time of the lattice which therefore cannot react coherently.

6.3 Dr S. Koonin pointed out at Erice that the reaction p + d ---> 3He has a much higher probability than d + d fusion and there is some H2O in the heavy water used by Fleischmann and Pons - however this is only 1/2 %.

7. REGIONALISATION OF RESULTS; CORRELATION BETWEEN RESULTS AND INFORMATION AVAILABLE.

There are striking variations of the ratio of confirmation to null results from one region to another. In North-western Europe (Britain, Switzerland France, Germany) there are only null results whereas in Italy there are two press conference releases announcing confirmation and I have heard of three other groups with confirming results (two neutrons and one protons).

In Eastern Europe, Asia and Latin America only confirming results have been announced. In the USA, the North-East plus major labs at Livermore and Los Alamos have reported null results while elsewhere the results are confirmations.

It is interesting to compare this with the information available in each region to see if there is a correlation with the results. Initially most of the world welcomed the information that the dream of energy with little pollution, had some experimental evidence, though doubts were expressed in a few papers, particularly the New York Times and some Western European papers. Responsible journalists in these regions continued to express doubts. As an example of the effect of the media on opinion, Don Perkins offered his first year class odds of 100 to one that there was nothing in the reports of heat generation by Fleischmann and Pons - and no one would take the chance of losing one pound in the hope of gaining one hundred from their professor. In Italy on the other hand, the first reports from Frascati that a new technique had been developed, were extensively reported on the front pages. In the rest of the world outside of the USA, I do not have enough information. In the USA, the New York Times has an important influence in the North-East and has expressed sceptical doubts and reported negative results.

The State of Utah is exceptional. Only confirmations seem to be reported. The economy is not in good shape and there are great hopes that cold fusion could change all this - as California has its Silicon Valley, Utah will have its Fusion Valley. The State has voted $5 million for fusion. The Office of Naval Research, ONR has voted an additional grant of $400 000 for the University of Utah team over the next 32 months nearly doubling ONR support for Pons's laboratory (Nature 20 April). And now Fleischmann and Pons (or the University of Utah) has requested $25 million from Congress to set up a realistic device to generate energy (Le Monde, 28 April). Some people are very firm believers in cold fusion like the person who taught Prof. Fleischmann at Imperial College and who now is a professor in the States. His group held a press conference announcing confirmation. He believes that only women "will make the world see sense and force governments to produce hard cash for an environmentally ideal fusion process". Hence he has written "to Jane Fonda to ask her to lead a campaign, America wide, to persuade Congress of the need for strong financial backing for nuclear fusion" (Mail on Sunday, April 16). His University has since retracted confirmation.

This possible correlation between the climate of information available in a region and the results obtained in that region, must be considered tentative as it is based on only partial information and a fuller time study is needed.

There is another regional divergence that is even more worrying. There seems to be a slight tendancy for chemists in the USA to be in favour of Fleischmann and Pons. Thus Nature of 20 April reported that at the American Chemical Society meeting in Dallas "chemists welcomed the prospect that cold fusion might represent a victory for chemistry over physics. Opening the special session, ACS president Clayton Callis said the goal of fusion as an energy source has remained elusive and that physicists' efforts at hot fusion using tokomacs and lasers were 'apparently too expensive and too ambitious to lead to practcal power'. To applause from the crowd, he added, 'Now it appears that chemists have come to the rescue'. In his speach, Pons joked about the high cost of physicists attempts at fusion by calling his own apparatus the 'U-1 Utah tokamak'. A picture showed a glass electrochemical cell inside a cooling bath made from an ordinary rubber kitchen bucket." This is sad. Not all results can be correct and clearly some must be wrong. In this difficult time to come, it is important that all scientists, no matter what their field of specialisation, should be supportive of those who produced wrong results and believed too firmly in them.

10. HISTORICAL PERSPECTIVE

In my studies of the history of Wrong Results in Science, or Pathalogical Science, a considerable number of the 12 conditions forming the syndrome, seem to be fulfilled. In particular we have passed Phase One where there are the original results and a few confirming results obtained quickly. In Phase Two, there seems to be confusion as there are about an equal number of confirming and negative results. On a world-wide basis, we seem to be in that phase now, but in some parts of the world we are already entering Phase Three where all the results are negative and no one believes in the effect, except the original discoverer.

In the past, the originators of the effect have not retracted. In the classic case of N-Rays, Prof. Blondlot was exposed rather cruelly by R. M. Wood, an American scientist visiting Europe who was asked by the Royal Society to call in on Blondlot and investigate. A year afterwards Blondlot published a book describing positively N-rays. An exception was the great German scientist Prof. Paneth who believed he had found helium from the fusion of hydrogen in palladium - he investigated further and published in 1928 very detailed evidence descibing where he had gone wrong - but he was a great scientist.

Historically speaking cold fusion seems to heading towards its end though there will still be strong advocates for some time longer. Some harm will have been done to Science through raising people's hopes too high, but there are also some positive sides - many have appreciated the importance of careful checking and control experiments, of not wishing a result too much. Also many have learnt something about other branches of Science that they do not normally consider. There is perhaps a better understanding of scientific method.

Doubtless this will enter into the History of Science. It is important that all act responsibly; there is no point in witch-hunting. Everyone makes mistakes and it is very important to be kind and understanding.

Douglas R. O. Morrison.

PS For the period 1 to 9 May, my Email address will be
DROM@FNAL

1From: VXCERN::FNAL::DROM 9-MAY-1989 01:35:59.53
To: VXCERN::DROMCD
CC:
Subj:

Dear E632 and WA84 Collaborators and Friends, 7 May 1989.

COLD FUSION NEWS No. 12.

REPORT ON AMERICAN PHYSICAL SOCIETY SESSIONS.

1. INTRODUCTION

The APS planned to hold a special session on Cold Fusion on the Monday evening, 1st May, but as there were many papers, this was extended to Tuesday evening as well. The Monday evening session started at 19.30 with some 1800 people and finished at 00.30 with fewer but still hundreds. Tuesday evening started with several hundred and finished at 23.30 with a few dozens. I tried to attend all talks, but after my survey talk at the start on Tuesday, I left for a while to hear the talk of Randi who was receiving a prize for "Promoting Public Understanding of the Relation of Physics to Society".

Here will concentrate on three talks, by M. Gai of Yale-BNL, Nathan Lewis of Cal. Tech, and W. Meyerhof of Stanford, which explain most of the problems in evaluating the claims of positive results. Other results, comments on theory and some summaries will be given.

2. YALE - BNL RESULTS.

M. Gai described an experiment to measure neutrons and gammas. A pentagon containing 4 cells was surrounded by 5 pentagonal neutron detectors containing NE213 scintillator and there were two sodium iodide counters outside to count gammas. The whole was enclosed in a large box of borated paraffin inside which was another box of borated concrete and two veto counters were placed above the detectors. The efficiency of the neutron counters was 1% and of the gamma detectors 0.1% at 5.5 MeV. The neutron counters were calibrated with 252Cf and 239Pu-9Be sources and the curves shown gave sharp peaks unlike the curves shown by Jones et al. for 2.9 and 5.2 MeV neutrons.

Nine different electrodes were used made of Palladium or of Titanium and prepared in a variety of ways, e.g. Pd annealed in flowing Argon at 1000 degrees, or titanium powder. Also nine different electrolytes were employed, 5 covering LiOD with different % of D2O and 4 were a soup of salts which seemed to me to be very close to the "soup" of Jones et al. (and adjusted to a pH < 3).

The gamma spectrum showed no peak at the d + d or at the p + d fusion to helium values of 23.77 or 5.49 MeV resp.

As a result of cuts from the veto and software, they were left with only two neutron events (which they named appropiately) after 10 hours running. Their background counting rate was only 2 counts per hour.
This gave the following limits at 3 sigma;
p + d gammas < 1 E-22 fusions per atom pair per sec.
d + d neutrons < 1 E-25 fusions per atom pair per sec.
This means that their neutron counting rate is one hundredth of that of Jones et al. and is one millionth of that of Fleischmann and Pons.

It had been suggested that alphas might initiate fusion so they attached a 5 MeV alpha source to an electrode, but no extra counts were recorded.

3. CAL. TECH. RESULTS.

Nathan Lewis gave a brilliant talk. He is a chemist and leads a team of chemists and physicists. They did experiments with palladium electrodes and with LiOD electrolytes. They looked for gammas over the range

20 KeV to 30 MeV and found no signal. They studied neutrons, tritium, helium and calorimetry and most importantly they reconstructed the calorimetric analysis of Fleischmann and Pons. They asked for detailed descriptions of the Utah cell but were not able to obtain them by asking, and so did some detective work to reconstruct it (e.g. from photos of the lengths of people's arms to the elbow they derived the cell dimensions!).

In the neutron and photon experiments, they used 7 different forms of palladium electrodes (e.g. cast, heated to 300 degrees under vacuum). Fleischmann and Pons calculated their flux of neutrons per hour per cm3 of Pd as 1.2 E8 whereas Cal Tech found less than 10 per hour that is more than ten million times less.

For tritium they tried a variety of electrolytes and also normal H2O. They found a maximum of 6400 tritium atoms per cm3 per sec. and this was mainly due to subtle chemical interference that generates chemiluminescence that gives tritium signals.

For helium they found less than 1 E-7 but noted that there was a very large amount of helium in the air which accounted for their value. (comment - Paneth and Peters in 1928 finally got down to a much lower 1 E-10 cm3).

When Cal Tech did their calorimetric experiments, they found that they could derive both positive and negative excess power depending on the details of the experiment and the method of calculating used! To try and understand this, they did some more detective work to try and reproduce the calculations of Fleischmann and Pons.

They showed a new version of the table 1 of F & P. For example taking the first rod of 0.1 cm diameter, they calculate;

Input power = 0.0714 watts
Output power = 0.0402 watts

Excess power = 0.0312 watts = -44%

Calculated Excess power = -56%

Effective excess power = +10%

To put this into words, the cell was consuming energy, but the F & P calculated it should have consumed still more energy, and therefore there must be some new source of power! Or as Nate Lewis said, it is a refrigerator which cools less than expected!

This can be understood when it is remembered that there is power consumption when the water is electrolised into deuterium and oxygen.

In the above example, the excess heat produced is 0.0075 watts as in table 1 of F & P. In the published paper, the claims of heat excesses of greater than 10 watts per cm3, depend on some extrapolations.

But Cal. Tech did observe some positive excess heat. This they investigated and found that F & P did not stir their electrolyte as they said it was not necessary as the bubbles did the stirring for them. However this problem has already been studied by Shirley and Brenner, J. El. Chem. Soc. 105 (1958) 665 where it is shown that there are temperature gradients if one does not stir, as the cathode is hotter than the anode. Cal Tech then moved their thermometer around and found that according to the position of the thermometer, they could obtain positive or negative excess heats (comment - think this is because one uses Newtons Law of cooling to evaluate the heat produced).

Lewis et al. also found a fault in the calculation. F & P do not give a certain voltage difference (comment - think it is at the face of the rod), but appear to assume a value of 0.5 Volts. However the electrolysis cannot procede unless this voltage is at least 0.8 Volts, and Lewis believes the true value was 3 1/2 to 4 Volts.

4. Prof. W. E. MEYERHOF

Prof Meyerhof is at Stanford and visited Prof. Huggins's lab where he had claimed to confirm Fleischmann's and Pons work by observing excess power. He studied the apparatus carefully and then with D. L. Huestis and D. C. Lorents of SRI, they made calculations with a theoretical model of the temperature gradients and heat flows in Huggins' cell. They found that since the palladium rod was off to one side, there were serious temperature differences in the thermometer readings according to its height in the cell so that you could get different power excesses according to the position of the thermometer. He gave a delightful gentle but forceful lecture showing a number of graphs explaining the calculations. He then tried to show his calculations to Prof Huggins to avoid problems, but although he left messages everywhere, he was not able to contact him before he went to Washington.

5. OTHER RESULTS

There were a number of other results, two of which were confirmations of neutron production;

Dr. D Seeliger of the Technische Universitat of Dresden DDR, reported "an indication for a weak fast neutron production of 0.1 per sec. around 2.5 MeV which could be caused by DD fusion". Reading the paper he gave me, table 1 shows that they had six runs of one hour and overall found an excess of neutron events of 20 +/- 5 per hour. The only unusual thing I noted in their paper was that the "blind experiment" showed an abnormal variation of counting rates of between 34 and 194 neutron events per hour. They used an electrolytic cell witha palladium cathode. They state that "within the very crude limit of temperature control, no big 'extraheat' was observed. No melting of the palladium electrodes was observed".

Two Argentine groups reported, one confirming the original results and the other not finding any effect. Dr. Granada of Rio Negro said that they had a high efficiency (22%) neutron detection system. They used a cell with a palladium cathode and LiH as salt. "The dynamic response to low frequency current pulses was measured. Characteristic patterns showing one or two bumps were obtained in a repeatable fashion". In his talk, Dr. Granada preferred to call his result an "indication" Dr. A. O. Macchiavelli of Buenos Aires reported measurements of neutrons, but observed no effect and gave an upper limit of 13 neutrons/sec which is three orders of magnitude less than Fleishmann and Pons.

Dr Brooks of Ohio State had a good neutron counter with 1% efficiency and pulse shape discrimination. They find a limit considerably smaller than Fleischmann and Pons but do not feel their results can be compared with Jones et al. They also had muon anti-coincidence counters and observed no evidence of muon-induced catalysis of fusion. They did not observe any 4He or tritium production.

Dr. Hirosky of Rochester found a limit for neutrons which was 40 000 times less than Fleischmann and Pons. Their neutron counter had an efficiency of 2.5% and the counting rate was < 0.5 per sec.

Dr. Dickens et al. of Oak Ridge also used a shielded neutron counter with a cell with several palladium electrodes of from 0.3 to 15 grams. The efficiency of their detector was 13% and the background counting rate was 3 per min. They found upper limits many orders of magnitude less that Fleischmann and Pons and also one tenth of Jones et al.

Dr. Sur of Berkeley reported measurements of gammas and neutrons. The neutron counter had an efficiency of 0.4% and they found < 0.75 neutrons per sec corresonding to a limit of < or = 5.4 E-23 fusions per dd pair per sec. From the absence of the 5.5 and 23.8 MeV gamma lines they found limits of
for p-d fusion, < or = 1.8 E-23 fusions per pair per sec.
for d-d fusion, < or = 2.9 E-23

As I was talking with Prof. Jones, I did not hear the talk of Drs Cantrell and Wells of Miami University, Oxford, Ohio, but their abstract is entitled "Electrochemically induced Excess Heat in a "cold fusion cell with a Zr2Pd electrode" In the text it is said "The temperature dependence of the process is positive", but there are not enough details in the abstract to comment.

M. R. Deakin et al. of Florida State University had a thin window in their cell with Pd cathode, to allow the measurement of K X-rays from the Pd excited possibly by charged fusion products (mostly protons). The background was 3 counts per hour corresponding to fewer than 50 fusions per sec or a fusion energy release rate of less than 1 E-10 watts in the Pd cathode.

S. C. Luckhardt et al. of MIT had an Abstract stating that neutron and gamma ray emission and calorimetry were measured. The only result given in their abstract was that the neutron rate was less than 160 counts per hour, that is substantially below the level of 40 000 per sec of Fleischmann and Pons.

In his survey, Morrison discussed the results from the Institute for Alternative Energy at Frascati in particular as they are not well known in the States. They had produced two graphs which appeared to show significant neutron counts and which they attributed to "dynamic" or "non-equilibrium effects", that is when they changed the conditions appreciably, there were neutrons produced. There were three problems with the first graph. (a) there seemed no logical correlation between the adding and removal of liquid nitrogen, (b) the counts seemed to be quantised, being either about 20 or about 40, (c) among the groups of counts of 20 or 40, there were many 10 minute bins with no counts or only 1 or 2. The authors postulated that this was in connection with the dead time of 5 microseconds and the neutrons were possibly emitted in very short bursts which produced the observed effect. However someone calculated that this would imply that the neutrons were emitted in bursts of a million at a time and then nothing which would be hard to reconcile with the few changes made. The second graph showed an impressive number of neutrons starting to be counted 3 hours after the cell was removed from a liquid nitrogen bath, and this counting rate rose to some 300 counts per 10 minutes, then declined smoothly to the background rate of a few per 10 minutes at 11 hours after time zero. There are two problems with this, (a) The time zero is not the start of the experiment - the experiment began a day earlier when pressure and low temperature were applied, this raises the question as to why this change of conditions at -24 hours did not cause a comparable emission of neutrons, (b) if the zero counts observed in the middle of the "dynamic" conditions in the first experiment were true, then why did no similar effect be observed in the second experiment? the suggestion of Dr.Williams of Harwell is that removing the cell from the liquid nitrogen bath could have affected the BF3 counter which are known to be very temperature and humidity sensitive (comment - normally there is lots of noise so the discriminator level is set just above this, but if the temperature and humidity conditions change, the noise level can change and appear to cause counts).

This survey gave disturbing statistical evidence that of the results reported, there was a significant correlation of the results with the region of the world. Further this effect seemed to be related to the climate of information available - thus if there was general expectation that the initial reports on cold fusion were true, then all the results reported were confirmations (except one - Buenos Aires, and the results were announced to the press). On the other hand, in those regions of the world where concerned scientists and responsible science reporters had been in close contact, then all the results reported were negative (except one - Stanford). (One is trying to improve this situation by disseminating information freely). Thus it appears that if one wishes to understand the cold fusion phenomenon, pure science alone is not enough.

Dr. Genet of the Institute of Nuclear Physics of Orsay in France told me privately that a group of radio-chemists and physicists had been looking for neutrons and gammas from a cell with a palladium rod, but could find no effect, their limit for neutrons being about 3 per sec.

6. THEORY

Steve Koonin of UC Santa Barbara gave the first theoretical talk, though he started by pointing out that in a pre-preprint, Fleishmann and Pons had shown a graph of the gamma spectrum which was different from that in their paper, the first having a low energy tail and the second a high energy tail. He criticised the Fleischmann and Pons experiment and noted that no one had been able to find the secondary effects of the large energy release that they had claimed. He roundly condemned them and was strongly applauded and later excess power. He studied the apparatus carefully and then with D. L. Huestis and D. C. Lorents of SRI, they made calculations with a theoretical model of the temperature gradients and heat flows in Huggins' cell. They found that since the palladium rod was off to one side, there were serious temperature differences in the thermometer readings according to its height in the cell so that you could get different power excesses according to the position of the thermometer. He gave a delightful gentle but forceful lecture showing a number of graphs explaining the calculations. He then tried to show his calculations to Prof Huggins to avoid problems, but although he left messages everywhere, he was not able to contact him before he went to Washington.

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M. R. Deakin et al. of Florida State University had a thin window in their cell with Pd cathode, to allow the measurement of K X-rays from the Pd excited possibly by charged fusion products (mostly protons). The background was 3 quoted, but at the Tuesday press conference did not repeat this although the panel was invited to do so by a reporter. He also reported that the claims of Dr. Hagelstein of MIT which appeared to "understand" cold fusion had been withdrawn.

Prof Rafelski wrote in his abstract that the fusion rates observed by their BYU experiment "could readily be explained by combination of standard nuclear physics data and WKB penetration integrals in the metal lattice environment".

There were a number of theory talks starting from the basis that experimemental results must be correct and therefore need explaining. Having reported this, details can be obtained from the book of 40 Abstracts.

Ming Li of Maryland examined more closely the suggestion of Koonin that fluctuations could enhance the fusion rate. He examined the cases of heat generation in Jupiter and of the results of Jones et al. and found that the fluctuations are of such a magnitude that they are unlikely.

7 CONCLUSION ON FLEISCHMANN AND PONS REPORTS.

In view of the results presented at this APS meeting it was generally considered that all 4 results (calorimetry, neutrons, gammas, and tritium) of Fleischmann and Pons are mistaken. An exception to this view is held by Prof Rafelski. At the Tuesday press conference Prof. Jones asked the panel to vote if they believed in the Fleischmann and Pons results. The vote was eight to one, the one being Prof Rafelski who said it was too soon and there might still be something in it. This unique viewpoint gained him some media attention and he was featured on the CBS news saying this, unfortunately they subtitled his statement with my name - do not know which of us was the most annoyed by this mistake!

8 CONCLUSION ON JONES ET AL. RESULT

Prof. Jones also asked the panel to express an opinion as to whether we believed in his result. Only a few of us said they did not believe this result. In view of the fact that on Friday after my talk at Fermilab, a member of the audience said that he had been at Prof. Jones's lecture at Argonne that morning and he had said that there were no experimental results in contradiction with his BYU result, would like to list the relevent experimental results;

Yale - BNL	found less than one hundredth of BYU neutron rate. They used titanium and a "soup" of electrolytes similar to that of BYU
Cal. Tech.	found less than one thousandth of the BYU number. They used palladium and LiOD, but it is hard to believe that this change could vary the result by a factor of 1000
Bugey (Fr)	found less than one hundredth of the BYU result. They used a Palladium electrode. Again it is hard to believe that this could change the result significantly.
AT&T	found less than one tenth of BYU using palladium rods and LiOD
Oak Ridge	found less than one tenth of BYU using palladium rods.

Note that prof. Jones has said that there was no special magical factor that was missing from their description.

To sum up there is no experiment that has been carefully performed and has controls and blind runs, which has given a positive identification of production of neutrons. Note the statistical evidence presented in the paper of Jones et al. needs correction for the scaling of the background (result of conversations with Prof. Jones).

In the 27 April issue of Nature there is a strong condemnation in an article of the papers of Fleischmann and Pons saying that they had not "carried out the rudimentary control experiment of running their electrolytic cells with ordinary water rather than heavy water". Prof. Jones was very unhappy with this as their preprint and published paper both said they had done so. He has received an apology from the person who had worked on his paper. Although Nature stumbled on this one point, it has been one of the very few sources of information available that has responded quickly and responsibly to the spate of media reports announcing cold fusion which had raised popular expectations too high. So despite this aberration, I think I will start ordering Nature.

8. FUTURE MEETINGS

At the press conference, Steve Koonin said that there was a conference of electrochemists starting in a few days time in California that was unusual as it invited only those who had positive results or confirmation of Fleischmann and Pons results to attend. He protested strongly. Prof Jones said he had been invited although his results were in disagreement with Fleischmann and Pons. Later heard that Dr. Lewis had been invited to attend.

Prof. Rafelski has said there is a meeting on cold fusion at Santa Fe but have not seen any announcement.

9. FUTURE

For almost all those who attended the APS meeting, the subject is closed in urgent scientific terms. There are a number of interesting scientific questions that have been raised that it may be interesting to clear up. However there are other problems. It is important for the chemists and physicists to work together. The public image of Science and scientists has suffered (e.g someone told me that he used to believe that doctors never made mistakes, now he thinks he should feel the same about scientists). Some say that the public has seen Science in action, repeating and correcting experiments quickly - partly true. A major lesson is that we should not hold press conferences before doing all checks and controls. But maybe the great lesson is that we all make mistakes sometimes and therefore should be tolerant and sympathetic.

Douglas R. O. Morrison.

PS There are certainly some mistakes in these notes though I have tried to have them checked - my apologies.